Philosophy of Science

VOL. 6 April, 1939 NO. 2

Virtue and the Scientist

BY

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IN THE practice of science lies the key to virtue. The proposition I have enunciated is not an obvious one. Its contradictory could conceivably be true. One might, for example, look upon the practice of science as a diabolical way of blinding one to the charms of virtue. One might look upon the practice of science as a deliberate plot to efface virtue, destroy it. Worse, one might look upon the practice of science as entirely apart from the issues of good and bad, of right and wrong, or of virtue and viciousness. The practice of science may be conceived as blind, itself vicious, or irresponsibly indifferent.

The proposition I have enunciated is not a masked definition, a rule of syntax, or a convention. One should not try to read it: the practice of science = the key to virtue. One should not try to read it so that whatever statements about the practice of science may turn out to be they should always be formally convertible into statements concerning something called "a key to virtue" as to guarantee a necessary and invariant relation between them through the chains of an unbreakable convention.

The proposition I have enunciated is an assertion concerning a state of affairs. As such it is analyzable into assertions concerning the "practice of science" and the other terms used. It is a typical scientific assertion and is entitled to the general rights and

privileges of all such assertions, and is subject to the general restrictions of all such assertions. It may have a few *special* privileges and restrictions as well, if it can be shown to belong to a science, let us say, the science of scientific behaviors or the science of virtuous behaviors, which has its own *special* characteristics. The general privileges of a scientific assertion are: to be verifiable in some not unusual sense, to be intelligible in terms of experiment, to be directed toward truth as an ideal; the general restrictions are: to be only probable, to be relative to contexts, to have some concrete reference. A special privilege or restriction of my assertion may be that it applies only to human behaviors and hence must carry the special character of being an assertion of a *social* science.

These preliminaries aside I can turn to another set of considerations which are also of a preparatory character. I feel bound to refer to the virulent disagreement among scientists themselves and among outsiders who interest themselves in science as to the scope of the activity of science with reference to the entire human scene. The disagreement has become especially sharp in the last few years. Obviously it reflects the rising conflict between progress and reaction. On the whole those who are "isolationists" with regard to science, tend to reactionary political views. In specific cases of some well-meaning individuals this is very unfortunate. They mean to be "neutral", but neutrality invariably turns out in practice to be a tolerance of the supremacy of evil over the good. And that is itself evil.

The "isolation" of science from the general field of action or behavior begins, as a rule, quite innocently. In the effort to explain the nature of the work of a scientist a philosopher of science might select only the most necessary characteristics of the scientist-in-action, portraying him either as an experimenting automaton or a logical machine, and forgetting to revivify him after the analysis. There results a notion of the division of people into "human beings" and "scientists". This is quite evident when someone turns to a scientist with a question the tenor of which is: "As a *scientist*, Mr. Blank, what do you think about such and such an issue?" Mr. Blank, if he has swallowed

the facile characterizations he has received of himself, can only say that "as a scientist" he is not permitted to think in certain ways. He will gullibly state that he cannot think "as a scientist" in terms of *values*. He will parrot something about "suspension of judgment" by which he means not suspension of dogmatic judgment, but suspension of all judgment as if everything were in a vacuum. He has unwittingly accepted the division of mankind into human beings and scientists. In this our philosophic readers may see a parallel to the taking of "mind" out of "nature", a fallacy which has drawn the protests of philosophers ranging from Engels to Whitehead. Once "mind" has been taken out of "nature" at least two mysteries must grow where one grew before. Once the scientist has been isolated from humanity at least twice as many cloaks for scoundrels may be found than there were before.

Another way in which science frequently receives an irreversible isolation is when it is thought of in terms of its results. What is in the annals of science, on the shelves of the library, is said to be the only science. It need not be consulted by anyone in particular, not even by scientists. It stands there alone in its austere beauty. If revisions or additions are introduced by the perversity of a haphazard progress, these themselves achieve this impersonal grandeur. Such science hates to be "applied", because thereby it runs the dangers of tests far more severe than the self-imposed ones of a smug methodology. A static theory eyes dynamic practice as the enemy of enemies. And every theory likes to think of itself as immortal.

Philosophers must introduce distinctions, but they are not generally in the habit of reminding others and even themselves that they are "merely" distinctions for some specific purpose. Like others, if I am to be an analytical thinker, I create dichotomies. I divide life, for example, into *work* and *play*. I must remember, however, that I have done this only for a certain convenience. Should I forget, I might be arguing after a while that science, which might fall under the head of work, can have nothing to say about play. My mistake would lie in transforming the *complementary* pair, "work" and "play", into *con-*

tradictories. Similarly, to continue with the example, my dichotomy under "work" is "action" and "contemplation". Under "action" I might list the serious activities of political action and the like; under "contemplation" I might include "science" and "philosophy". Again, if I forget the complementarity I intended between, let us say, "science" and "action", I shall fall into the false assertions that one cannot act on a scientific basis or be scientific about action.[1] There are, of course, actions nearly devoid of any reasonable scientific basis, and scientific matters-of-course almost free from the decisions of action, but these extremes show all the more the necessity of preventing a complete break. Reconciliation, and not divorce, is the remedy. Science, as L. A. White[2] has said so aptly, is *sciencing*. It is itself action of a certain kind.

The fallacy of setting up a contradiction between science and action has some curious offspring and other relatives. They can all be lumped under what is often mislabeled as "the social implications of science".[3] The dubious contributions which are served to us under this head divide roughly into (1) disclaimers that there is any issue in the question, (2) all sorts of charges of villainy against science and (3) all sorts of eulogies to the beneficent contributions of science. These deserve a brief discussion before I go on to my positive data for the assertion that in the practice of science lies the key to virtue.

The disclaimers that there is any issue to the question of the

[1] Those who wish to treat "science" and "action" as true opposites and seriously consider action unscientific, may, if they wish, conclude that science-in-action (e.g. scientific research) is unscientific! Or, I am afraid, they will have to deny the existence of research. I think they will compromise and say that there is nothing but *chance* in scientific research and that there's no more reason to expect truth or error in the choices taken by a research investigator. They will try to show that scientific research or progress is an *illusion*, because it is based on irrational (unscientific!) action.

[2] L. A. White, *Philosophy of Science*, Vol. 5, pp. 369–89 (1938).

[3] The expression "social implications of science" is technically inaccurate and otherwise inadequate to describe the situation that is meant. "Implication" is a logical term used in such expressions as "A implies B". Such a rigid, necessary connection cannot exist between a scientific discovery and its social uses or abuses, which may receive no realization altogether. All that can be meant is that there is some kind of mutual interaction between social conditions and scientific activity.

social responsibility of science are based principally on the rewarming of the old notion that a scientist need not concern himself with the application or practical meaning of his discovery at all. "Free" inquiry is interpreted to mean freedom from the requirement of the kind of test in practice which comes with "applications". The advocates of this view of freedom do not see that there is actually a possibility of the *obstruction* of progress by limiting the nature of verification to the circle of theoretical-experimental operations set up by some specific theory. They do not realize that "applications" are verifications or confirmations as important as any. Their truths shun their severest test where contexts cannot be bribed, so-to-speak. Grant to such people this dangerous assumption of the "freedom" of pure science, and their argument becomes simple. They will say: "Do not blame us for machine-guns, overproduction, anything. As 'pure' scientists (!) we are not concerned with applications". Strange, they may be right that one should not blame science, but they give the wrong reason. They admit that science is as impartial as a *tool*. Anyone may use it as he pleases for better or for worse. One can, thus, easily convert a tool into a weapon. Alas, this is true about certain of the *results* of science, but science is an activity, i.e. something much more than its results, and, as such cannot shake off social responsibility or involvement of a moral character. It is true that there is no use in raging against a tool, but science is not a mere tool.

If science be a mere tool, both charges of villainy and eulogies are only transfers of epithet. My "good knife" and "the bad knife" that cut me, are what they are only by virtue of my petulance or my enthusiasm. On the other hand, if science is functionally integral with social activity, then it is involved in the ethical appraisal of all social activity.

The charges, however, continue against science as a "cause" of social disruption, even when the critics realize that science is not a blind tool. What such critics have in mind are the tremendous *economic* effects of scientific progress, which, in spite of the protests of "pure" scientists, comes along with a full quota of applications. They will grant that economic changes in turn

affect science. The relation, I hope they will admit, is functional. They will, therefore, even drop loose talk about "causes". (Somehow, you see, we feel we can scold a "cause".) With the elimination of a too one-sided, anthropomorphic use of the term cause, we can hope that the scolding will stop.

Once the scolding about inventions, cold impersonal laws and the like, is waved aside as irrelevant to the issue of the ethical involvement of science, we can turn to the definite message of this contribution: in the *practice* of science lies the key to virtue. Inventions can be used as tools for better or for worse. "Pure" laws of science, e.g. the law of gravitation are indifferent to their use by scoundrels or by saints. Actual living scientists may be the dupes of evil men or the collaborators of great statesmen. But, I maintain, the method of science (its true and full practice) lends itself only to virtue. It does separate the sheep from goats, the virtuous from the vicious.

Consider a scientist who has decided to lie in recording his observations. I mean that he has decided to lie effectively, with no guile that would enable him to substitute words and get back to the truth again. He may record "black" whenever he sees "white" and vice versa, and thus be able to translate his record into a genuine one. Such masking is not lying and may be practiced only as a code. Suppose that he is genuinely lying and that instead of "black" he occasionally records "yellow" or "pink" or "pleasant" or "worth a dollar", as the spirit moves him. What are his chances of making a genuine discovery? They are very small indeed. They are "nil" for all practical purposes, even if formally one can occasionally deduce a truth from a chain of well-adjusted lies or by sheer accident of balancing errors arrive at a true inductive conclusion. A scientist bent on discovery cannot risk lying. Deliberate lying is *immoral* in the context of scientific behavior because it completely, or almost completely, disrupts that behavior.

Consider a scientist who fails to make a discovery from his data. He finds it necessary to re-examine his data. His first suspicion is that he might have misrecorded an observation or made a malobservation. He repeats observations in order to check up on a possible unconscious lie. The experienced observer

knows how vulnerable scientific work is at that point. He is far from being in love with mere data gathering. He knows that the data, however, will betray him more effectively than anything else. The *neglect* of *unconscious lying* is *immoral* in the context of scientific behavior, because it may seriously or completely disrupt that behavior.

Failure to check observations or to allow for systematic error is a form of immoral behavior. It is not amoral, since it has serious moral consequences. Measuring, which properly should be done with a full technique of checking on the possibilities of self deception, is often described as amoral, as unconcerned with virtue. On the contrary, proper technique in measuring is a highly moral act. It discriminates emphatically between truth-telling and lying. It even involves resolves and high courage to adjust observations to the widest experiences of humanity with regard to error. The choice of the principle of least squares, for instance, is a heavy moral responsibility for the scientist as a truth-telling reporter. He even has the further responsibility as a philosopher of science to examine rival *moral* principles concerning the adjustment of observations, and may worry himself into repentance over the choice of a wrong principle, wrong because it breaks on the rocks of experience.

The stupidity (evil) of lying is brought out most clearly by the scientist's conduct in the gathering and in the adjustment of observations.

We know also that no matter how careful we have been there are "residual errors", that is, errors for which adjustment can be made only as theory evolves. New material of experience may have a different tale to tell. A larger context will reveal what has been called a "constant error". We are not through with moral responsibility. The moral responsibility of a scientist in the first place, therefore, is not to be dogmatic, i.e. to know that he does not know all. In the second place, he is bound to seek new experience and to expand his horizon. All this he must do in order to eliminate the more secret strongholds of error. The elusiveness of fact dictates this morality. Research for greater generality is a moral obligation.

The search for lurking error leads one on and on to the examina-

tion of the fundamentals and the presuppositions of the sciences individually and collectively. This is a tremendously long-range project and furnishes moral propulsion sufficient to satisfy the hungriest of idealistic souls. The realm of ignorance seems sufficiently inexhaustible to satisfy the crusading of the most infinity-loving moralist. The rewards of knowledge and of understanding can nonetheless keep piling up. Morality can thus thrive endlessly on receding ignorance.

The scientist's neverending search for truth is true to Socrates' doctrine that the only evil is ignorance.

Consider now a scientist who wants to isolate his activity from the similar activities of other scientists and from the activities of his fellow men. Such a Robinson Crusoe can have his natural island but no savages, and surely no Man Friday. It must be admitted that he could carry on something that could be considered faintly scientific and that only in a narrow sense. He could note to himself perhaps a little more than if he were a lone wolf on an island. He is a human being after all. For a proper test of his isolation, however, I failed to insist above, he should have been brought to the island at birth, or mercifully, a little later. Crusoe, it must be remembered, had a good social past. Our hypothetical scientist, too, was permitted to reach the island in a high state of civilization, a more or less complete social being. He would keep on doing something resembling the scientific on the funded knowledge of his past. He would not be strictly isolated. He would be merely an ingrate or a renegade to his past, and he would surely deteriorate as time went on, or would miss the advantages of collective action and means in the pursuit of knowledge. Were another younger being to arrive at his island he would have to train him, tell him at least about what he had discovered, and would thus become "social".

In the pursuit of knowledge total isolation, genuine isolation at the very start from all social influence, would reduce inherited knowledge to a zero. A discussion of partial isolation would also yield the point that science must be social. For, if it were to be argued that our social inheritance is full of error, isolation of some sort might then do good, but it would have to be *isolation from social error*, and not isolation from social knowledge.

Socially accumulated knowledge is the repository of moral worth because it is immeasurably greater than any knowledge an individual can discover without a society. A society is also needed for the optimum performance of the highly moral act of verification or confirmation.

The scientist who realizes the social nature of his truth-seeking enterprise is obligated, therefore, to act so that the sum total of knowledge is increased. His moral functions include first and foremost the devising of languages that will be understood by all his fellow scientists and in a broader way by all those whose morals rise to the search for truth. I know of nothing more democratic than arithmetic. Nor is there anything more moral. It is a universal language, and it demonstrates the virtues of democracy, human brotherhood and the genuine liberation of knowledge. So taken the law of gravitation is also eloquent of the democratic virtues. Every harmony it discovers in the stars suggests the harmony that the method of its discovery shows to be possible in the behavior of men.

Let us select yet another scientist who perversely might insist that he would still run counter to morals by working only for himself. Suppose he were to try to lengthen his own life by one year. He would quickly find that he would need help. He would need the libraries of the world, the past efforts of thousands of men each of whom depended on the thousands before them. He would need laboratories. These would have to be built. He would need materials, assistants. These could not come from the thin air or without intense human effort. He would need the entire paraphernalia of civilization, the teachers, the doctors, yea the artists. He would need everyone in order to accomplish what he conceived to be a merely selfish act. He would have to keep everyone going. And, thus, selfishness would turn willy-nilly to altruism in the *intelligent* pursuit of its aim. Such is the moral fibre of scientific acitivity.

It is time to repeat: in the practice of science lies the key to virtue.

Examples have been taken by me from the practice of science when it is on its best behavior. My thesis is that it can serve

as a model for all behavior. It amounts to a proposal that we model our general ethics on the ethics of research. I have not stopped to discuss the numerous meanings of "virtue", not that it may not be of interest or even of some profit to list the virtues and opinion about the virtues, but because I wish to leave the definition of virtue as an open research problem. I hope that the more we learn about successful scientific procedure the closer we will get to a characterization of virtuous conduct. Only an ethics of science, I believe, can harmonize fact and value.

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The Evolution of Theories of Space-Time and Mechanics

BY

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IN THIS paper I attempt to trace certain aspects of the evolution of theories of space-time and mechanics as revealed by a brief comparative study of Newtonian theory, Robb's theory, general relativity, and Milne's kinematical relativity. The first object is to emphasise how each theory leaves us in a position in which the succeeding one appears as a perfectly natural next step in the development of ideas. The second object is to show how, in spite of superficial differences in character, the theories all necessarily possess the same general structure constituted by the presence of hypotheses, from which certain general mathematical relations are deduced, which in their turn are used to predict relations between observable quantities.

This study may claim an interest of its own, but it is presented also for a further reason. It has been contended[1] that theories like Milne's represent a fundamentally new outlook on the part of some theorists, in that such theories are purely mental constructs divorced from experience of the physical world. We shall see that on the contrary Milne's theory is easily brought into line with the others in such a way that this criticism is neither more nor less true of it than of the rest.

[1] See in particular H. Dingle, *Nature*, *139* (1937), 784–786; *Philosophy*, *11*, (1936), No. 41.

In the general discussion of the theories they will be used to illustrate the view that the constituents which are of physical significance are sets of mathematical relations, coupled with sets of rules of interpretation, and that these yield, after observational test, descriptions rather than explanations of physical phenomena. It will also be recalled how this view leads to simple criteria for comparing the merits of different theories. Further, in this view, the initial hypotheses from which the mathematical relations are deduced do not ultimately have any direct physical significance. Their rôle in the construction of the theories will be briefly discussed.

Allusion will also be made to the recent work of Eddington and the conclusion he draws from it about the nature of physical theory. This work does actually appear to be on a different footing from the other theories, provided this conclusion of Eddington's be accepted. While the other theories predict the behaviour of entities whose existence is assumed in their rules of interpretation, Eddington's theory claims to predict this existence as a consequence of our laws of thought.

We start by considering the individual theories in turn. By assuming a general acquaintance with them we seek to avoid having to give systematic summaries. But one cannot help having to recall explicitly for the purposes of the discussion a number of very well-known results.

CLASSICAL MECHANICS

The system of classical mechanics presupposes a definite theory of time and space which there is no need to recapitulate. It then starts with the three laws of motion. Now what is important for us here is the fact recently stressed afresh by Filon[2] that "the three laws of motion are not experimental laws." In fact they are stated in terms of quantities (mass and force) for which no antecedent means of measurement are prescribed. However, taking these quantities as indefinables at the outset, we can deduce from them a complete system of rational mechan-

[2] L. N. G. Filon, *Mathematical Gazette*, *22* (1938), 9–16.

ics. This is a purely theoretical mental construct. But as Filon shows, we may compare the deduced properties of the theoretical system with experimental properties of actual physical systems, on the *hypothesis* that there is a one-to-one correspondence between these properties, and show how actually to measure mass and force. We can then test this hypothesis by further comparisons between theory and observation. Since there is agreement[3] in all macroscopic laboratory tests that have been applied (and very close agreement in astronomical tests as well) we regard the theory as a "good" one. The meaning of the last statement will be discussed a little further below.

There is a rough but instructive analogy between the relation of Newtonian theoretical mechanics to the experimental mechanics of physical systems, and the relation of euclidean pure geometry to the experimental geometry of physical systems (not to be confused with more sophisticated uses of the term "geometry" below). For consider the problem of finding the geometry of a rigid plane explored by means of a rigid scale as in naïve elementary "practical" geometry. We do not discover the *axioms* of euclidean geometry as experimental laws. But if we start from these axioms, which of course involve indefinables, we may develope a purely theoretrical geometry. If we compare certain deduced theorems with experimental results, we first see to what components of the physical figures the indefinables correspond. Then with this correspondence we find that the theoretical system gives results in all cases in agreement with observation (within the limits of experimental error). So we may say that we have discovered that the geometry of the rigid plane is euclidean, just as we might say that we have discovered that the mechanics of ordinary mechanical systems is Newtonian.

In both cases the axioms have of course been so designed that the theoretical system may reproduce properties corresponding as closely as possible with experience. But, as we have seen, they are not themselves experimental "laws." In fact, par-

[3] We use "agreement" in the usual elementary sense, and do not here go into deeper questions of the significance of "experimental error."

ticularly in the light of Filon's discussion, we may take over a description applied by Milne to his theory[4] and apply it to *Newtonian* theory (without any implication that the fields of application of the two theories are indentical) and say: Newtonian theory *can also be regarded as an abstract geometry.* The propositions which follow *are true of entities which are the subjects of the propositions* expressed by the laws of motion *independently of whether such entities 'exist' or not, just as propositions of geometry are true of the subjects of certain axioms without the 'existence' of these subjects being relevant. Actually if it were not possible to realize in nature*[5] forces and masses *the subject of* classical mechanics *as a branch of experimental philosophy would not exist.* The words in italics are quoted from Milne, and the other words merely replace the terms he used for specific reference to his own theory.

As usual care must be taken to avoid confusion arising from the accustomed use of the same words in different contexts of the subject. For whether or not a theory may be regarded as an abstract geometry in the sense just described has nothing whatever to do with the use of what are loosely called "geometrical methods" in working out the theory. Thus, for example, the theory of general relativity as usually formulated cannot, as we shall see, be regarded as an abstract geometry, even though it uses geometrical methods throughout. Again, by "laws of nature" we most commonly mean an empirical law arrived at by a process of induction, but we have just explained that, though Newton's axioms are usually called "laws," they are not laws of nature in this sense.

Next consider Newton's law of gravitation. Had the technological difficulties not been too great, this is a law which might have been arrived at empirically for terrestrial phenomena and then extrapolated to astronomical phenomena. For it is stated in terms only of mass and force, and makes its appearance at a

[4] E. A. Milne, *Relativity, Gravitation, and World-Structure* (1935), 30. (This book will be referred to below as *W. S.*) See also H. Poincaré, *La Science et l'Hypothèse* (1908), 5.

[5] More properly, to describe certain phenomena occurring in nature in terms of *forces* and *masses.*

stage when these may be regarded as measurable quantities. But as used by Newton it was in fact a hypothesis which was tested by working out its consequences when combined with his laws of motion. All the work that has been done on planetary motion, lunar motion, cometary motion, and tidal theory can be regarded as providing tests of the Newtonian theory *i.e.* of the combined hypotheses regarding time and space, laws of motion, and gravitation. When Newton claimed that he did not make hypotheses he presumably meant speculations which were not, or could not be, tested by observation, either directly or by their deduced consequences.

We have now to recall the reasons which led to the search for a "better" theory, reminding ourselves that these reasons may not have been explicitly stated by those engaged in the work, but need only have belonged to what Whitehead would call the "climate of thought" in which the search took place. First, Newton deduced the planetary motions as consequences of his laws of motion *together with* his law of gravitation. The next step would be to see if both of these could be got as consequences of something still more fundamental. For the plausibility of such a step is evident as follows: Newton's second law of motion tells how the motion of a body is related to the force acting on it, and *force*, we have noted, is not something of which we have antecedent knowledge but is defined through the laws of motion. Then the law of gravitation says that if the body is in the gravitational field of the Sun (for example) the force is so and so. Therefore, as far as the application to planetary motion goes, we bring in the whole general idea of force only to replace it immediately by a very particular kind of force, and further, we use this only in association with the laws of motion themselves. So, in regard to this application, it is natural to expect that there might be some more compact general principle which would enable us to deduce the path of a free body in the neighbourhood of the Sun, without creating the intervening notion of force at all.

(To anticipate, this is precisely what the theory of general relativity achieves, amongst other things. Incidentally, it is often said in this connection that the latter theory resolves the

old difficulty about "action at a distance" in gravitational problems. But the reasons sometimes given are misleading. The reason would appear to be merely that, had the theory of relativity been discovered before Newtonian theory, the difficulty would not have been there to be resolved, for the whole concept of force would have been absent. It is of the utmost importance to realise that we are apt to create physical problems out of a too deeply ingrained habit of thinking in terms of a particular *theory* of physical phenomena. Force at a distance was originally introduced purely as a mathematical device, but afterwards extensive theories of an aether were invented to "explain" it. This point will be returned to when we consider the rôle of the "intuitive view", and we shall see that the attitude of thought suggested at the end of the paper is advocated partly because it avoids such complications.)

Second, when it became realised through the work of pure mathematicians that euclidean geometry is far from being the only self-consistent geometry, it became a matter for speculation as to whether it was necessarily the geometry which should be applied in physics. But further, Newton's laws of motion and gravitation are postulates added to his more or less implicit postulates about the geometry of "space." Now it is not hard to see how there should be some interdependence between these two sets of postulates. For what we call the geometry of space is something we discover by making measurements (in the manner described above for the case of the rigid plane). To make these measurements we necessarily employ material measuring instruments and material reference bodies. So the results of these measurements are bound up with the laws of behaviour of these material instruments; there can be no *physical* significance in a geometry which has no relation to the behaviour of matter. However, as long as we recognize this interconnection, it is unimportant whether we regard the geometry, which we discover, as a consequence of the behaviour of matter, or the behaviour of matter as a consequence of the geometry. The latter has happened to prove the more convenient for the mathematical handling of the problem. It is in fact the method of Einstein's general rela-

tivity theory, which it has become a commonplace to describe as the "geometrization of physics."

Third, the predictions of Newtonian theory were ultimately found to be in disagreement with observation in some few instances, and had this not been so we may question if any attempt would ever have been made to supercede it. The case of the advance of the perihelion of mercury is well-known.[6] Again, though initially the result of the Michelson-Morley experiment was considered in relation to electromagnetic theory, it soon became evident that it would affect theories of time and space, and of mechanics. Also there was an internal difficulty in classical theory in connection with Poisson's equation, which is merely one way of expressing Newton's law of gravitation. For Newtonian space is of infinite extent and there is no *a priori* reason for excluding the possibility of the existence of an infinite amount of material in it. However we find that we cannot consistently apply Poisson's equation to an infinite distribution of matter, which we would wish to do in order to test the consequences of the hypothesis that such a distribution can exist. So if we are to retain Poisson's equation, and so Newton's law of gravitation, we can only think of the universe as consisting of a finite amount of matter immersed in infinite space, in many ways an unsatisfactory concept.

Suppose now that we do make a frank attempt to geometrize physics. A geometry is a mathematical science, and a mathematical science is a set of theorems deduced from a set of axioms. Hence ideally the way to give physical content to the theorems would appear to be to give physical content to the axioms. That is to say, the mathematical axioms should express postulates about physical systems; the indefinable elements in the axioms should correspond to physical entities, and (in order that the theorems may express inescapable physical laws[7]) the axiomatic relations should express immediately perceived relations between these entities.

[6] But particular reference should be made to H. Jeffreys, *Scientific Inference* (1937) Ch. IX.

[7] Actually we subsequently conclude that no such theory is possible.

ROBB'S THEORY

The only existing attempt to work out an axiomatic theory of time and space appears to be that of A. A. Robb.[8] Its author does not claim for it the ideal status envisaged in the last paragraph; nevertheless we shall find it instructive to examine how far such a claim could be maintained.

Immediate consciousness of the "passage of time" is usually taken to represent our most elementary physical experience. Granting this, our most elementary observation about the physical world is that if we have direct consciousness of two different events A, B then we put them in a time order and say "B is after A" or "A is after B." Robb makes this the basis of his system and lays down:

"Postulate I. *If an element B be after an element A, then the element A is not after the element B.*"

We then say that A is *before* B. Here the "elements" (subsequently identified with physical "events") in the axioms are their undefined subjects, and the "after" relation is the undefined relation involved in them.

This first postulate satisfies our ideal requirement of expressing an immediately perceived relation. However, consider now a later postulate:

"Postulate V. *If A be any element, there is at least one other element distinct from A, which is neither before nor after A.*"

This admits the existence of pairs of events, such as A, C (say), such that if A is directly experienced by any observer, then C *cannot* be directly experienced by him. For otherwise that observer would be able to say that C is before or after A. Newtonian theory, through its initial assumption of universal time, denies the existence of such pairs of events. This postulate marks the first point of divergence from the latter theory. But now for our present purposes we have to notice that, though we could not proceed along anything like the desired route with-

[8] A. A. Robb, *Geometry of Time and Space* (1936), originally published as *A Theory of Time and Space* (1914) and summarised in *The Absolute Relations of Time and Space* (1921).

out this postulate or its negative, neither could be regarded as an immediately perceived relation. Further, since the physical interpretation of Postulate V states that no observer can exist who has direct experience of A and C, and since the negative of Postulate V would state that no event can exist, which it is impossible for any observer to experience, who also experiences A, neither can be regarded as an empirical law of nature. For no empirical law can *deny* the existence of anything whose existence is not logically impossible. Therefore Postulate V, when given its physical interpretation, ranks merely as an *hypothesis*.

Any assumption of uniqueness must be in the same position. For example, Robb has the following:

"Postulate IX (a). *If a be an optical line and if* A_1 *be any element which is not in the optical line but before some element of it, there is one single element which is an element both of the optical line a and the sub-set* α_1."

The definitions of "optical line" and "sub-set" need not be given here. For this Postulate is quoted firstly to show that assumptions of uniqueness are an essential part of the theory, and secondly as an example of a postulate having such a degree of complexity that it could not be claimed to correspond with any intuitive ideas about time and space. Once more, from the physical standpoint it becomes a hypothesis which can be tested only along with the rest by working out their consequences and comparing them with experience.

It is not our purpose to give a systematic summary of Robb's theory. We now recall merely that he builds up the geometry of his set of elements in the standard way of developing a geometry from its axioms. *Lines* and *planes* are defined, there being three sorts of each characterised by different before and after relations amongst their elements. Postulates are introduced which make the geometry four-dimensional, which give something like a parallel postulate allowing the setting up of congruence relations, and which render the aggregate of elements a continuum. Finally the possibility of introducing numerical coordinates, and thence of expressing the geometrical relations metrically, is deduced. Then it is shown how to interpret the

results in terms of physical measurements made by any particular observer.

The final results are found to be exactly equivalent as far as they go to those of Einstein's theory of special (or *restricted*) relativity. But Einstein himself did not claim to base his work on a rigorous system of postulates. For instance he took it for granted that we may attach numerical coordinates to events in space-time without analyzing the assumptions involved in this step. Robb's theory supplies this analysis, and also shows how measurement may be introduced without assumptions about perfectly rigid measuring rods or perfectly uniform clocks. In doing this it shows what a great deal is implied in any facile statement of a "relativity principle" to the effect that "all coordinate systems are equivalent." However, in its field of application to physical phenomena it is subject to the same limitations as special relativity; and it does not make the main advances beyond Newtonian theory the possibility of which was anticipated above.

Robb's theory is here discussed merely as an example of one developed from a complete set of explicitly stated axioms, being in fact the only example available. In his case a minimum of such axioms is used, in the sense that all are required and none can be deduced from the rest. The final theory does less than we wish, so we may conclude that a theory which would do all that we wish must be more complicated. Therefore it is probably safe to conclude that such a theory must require a more complicated system of axioms. So if the axioms of Robb's theory are, as we saw, too complicated to be admitted as immediately perceived and inevitable physical relations, and have to be viewed as hypotheses to be tested by their consequences, this must *a fortiori* be true of the axioms of a more complicated theory. We are thus led to suggest that the only way in which an axiomatic theory differs from any other is that *all* the hypotheses (apart, of course, from those of the formal logic employed in the deductions) are explicitly stated. Even this is not as considerable a difference as might be supposed, for we see by the example of Robb's theory that the hypotheses in general cannot be tested separately by

observation, but only through their consequences in the aggregate.

A possible course for further progress would now be to admit this view, but to seek a new theory by trying new hypotheses in the form of new axioms. In practice this would amount to relaxing some of Robb's postulates, in much the same way as we can get geometries different from Euclid's by relaxing the parallel postulate. But this methods offers too many possibilities to appear feasible.

To overcome the difficulty we may instead try the effect of using a "ready-made" geometry for space-time. For in Robb's method we proceed from postulates to geometry and from geometry to the physical interpretation. So the new proposal is to forego the first step, and to select on general grounds some known system of geometry, and then to see if we can learn anything by trying to attach a physical interpretation to the geometrical theorems. Actually, as we shall see in the next section, this is effectively the procedure employed in the theory of general relativity.

GENERAL RELATIVITY

This theory has never been placed on an axiomatic basis. Einstein himself in his original development[9] of it explicitly refrained from any attempt to do so (and his followers have remained loyal to his example!) The first stage of the theory is to represent space-time by means of a four-dimensional Riemannian space. (This gives at once as a pragmatic reason for the absence of an axiomatic development the great difficulty of formulating axioms for differential geometry.[10] Any system of axioms for general relativity would have to include ones corresponding to those of the differential geometry of Riemannian space). This is usually treated as a generalisation of the result that the consequences of the theory of special relativity may be

[9] A. Einstein, *Annalen der Physik, 49* (1916); *Principle of Relativity* (1923), 111–164.

[10] O. Veblen and J. H. C. Whitehead, *Foundations of Differential Geometry* (Cambridge Tracts, No. 29) (1933). See p. 24.

represented by means of Minkowski geometry, the generalisation being guided by the Principle of Equivalence and the Principal of Covariance.[11] Or use may be made of the arguments, extended to four dimensions, which Riemann himself gave for regarding what is now known as Riemannian geometry as a natural extension of euclidean geometry and for its possible applications in physics.[12] But either way we get only plausibility arguments which lead to the attitude, Let us try what consequences follow from assuming that the geometry of space-time may be a general Riemannian geometry instead of Minkowski geometry. That this step is a very tentative one is shown by the immense amount of research to which a further analysis of it can lead.[13] In particular the usual developments do not at this stage enter into the problem of what a system of coordinates in space-time means in terms of possible observations by an observer belonging to it. The whole thing is in fact an example of *hypothesis suggested by mathematical form*, a feature which is not present in any purely deductive theory, of which we say a little more later on.

The further development of the theory as far as it concerns us is briefly this: We note the existence in the geometry of geodesics and null-geodesics, and tentatively assume that they correspond with the world-lines of free particles and light-signals respectively. Then we note the existence of the contracted Riemann-Christoffel tensor G^p_q which is such that, using standard notation, the tensor divergence of $G^p_q - \frac{1}{2} g^p_q G + \lambda g^p_q$ is zero. If we write down side by side with the equations expressing this property, the general equations of motion of a fluid under any system of stresses and body-forces according to classical theory, we observe that the two sets mimic each other in general form. One component of this tensor occupies places in the equations corresponding to those occupied by material density in the classical equations,

[11] See A. S. Eddington, *Mathematical Theory of Relativity* (1922), §§ 16–18.
R. C. Tolman, *Relativity, Thermodynamics and Cosmology* (1934), §§ 72–74.
W. Pauli, jr. *Encykl. d. Math. Wiss.*, V2, 539–775. See §§ 52, 53.

[12] B. Riemann, *Math. Werke* (1876), 254–269; W. K. Clifford, *Math. Papers* (1882) 55–71.

[13] See, for example, H. Weyl, *Das Raumproblem.*

another set of components occupy places corresponding to momentum, others occupy places corresponding to stress. So we label these components "density," "momentum," "stress" and proceed to work out the consequences of this indentification. From it we can deduce in the first place as a limiting case the world-line of a free particle and verify that we do in fact obtain a geodesic. We next can write down the covariant generalisation of Maxwell's equations and similarly verify that the assumption that the world-line of a light-signal is a null-geodesic is consistent with them. Variants of this procedure are possible,[14] but the underlying method of introducing new hypotheses by means of a Principle of Indentification[15] is inseparable from the method of starting with a general hypothesis about the geometry of space-time.

Finally, any observer in the space-time can use *local* galilean coordinates, and in them interpret local phenomena as in special relativity. So we can express the properties of the space-time as relations between what are *observable* quantities for such an observer.[16] It is not until this stage is reached that the theory can be precisely tested by comparison with observation. At the intermediate stages it is only a species of qualitative comparison with the known general type of behaviour of various physical quantities which is invoked in the indentification with them of the various geometrical magnitudes.

Recalling now our list of desired advances beyond Newtonian theory we can notice briefly how far general relativity succeeds in achieving them. Firstly, as is well-known, it does yield planetary orbits (from the Schwarzschild solution of Einstein's field equations) directly, without the intervention of a "force" concept. Secondly, it avoids the separation of assumptions about space-time from those about matter. In it "there is no such thing as a body in space, but matter is an aspect of the space-time structure"[17]. This in turn leads to a sort of elimination of

[14] See A. Einstein, L. Infeld, and B. Hoffmann, *Annals of Math.*, *39* (1938), 65–100.
[15] Eddington, *op. cit*, 119, 222.
[16] See for example W. H. McCrea, *Zeits. für Astrophysik*, *9* (1935), 290–314.
[17] Veblen and Whitehead, *op. cit.*, 32.

the geometry from the final physical theory. For in the end we establish the relations of observable phenomena amongst themselves. For instance, in relativistic cosmology we can predict the relation between the red-shift of the spectral lines of distant nebulae and the apparent brightness of these objects, which is the sort of prediction we can test directly by observation. Thirdly, when tests of this general character are made we get closer agreement with observation than was obtained by Newtonian theory; we refer of course primarily to the three "classical" tests of the theory in regard to the advance of the perihelion of Mercury, the bending of light past the Sun, and the red-shift of light coming from a massive body. It seems true to say that no prediction of the theory has been contradicted by observation (within the errors of the observations). Also the internal difficulty in regard to the possibility of an infinite distribution of matter does not arise.

The way in which all this has been achieved can be viewed as follows. Nineteenth century mathematical physicists like Maxwell and Kelvin obtained results by employing mechanical models of the aether. Maxwell, at anyrate, regarded the models as no more than convenient intermediaries for suggesting mathematical relations between the physical quantities in which he was interested.[18] In much the same spirit general relativity has taken advantage of a *geometrical model* to suggest relations between physical quantities.

Now it might well be thought desirable entirely to dispense with the use of models. We might argue thus: General relativity starts at a later stage in the geometry than does an axiomatic theory like Robb's; alternatively we might now try starting at a later stage in the physics, i.e. with fewer, but logically more complex, physical hypotheses. This is in fact a possible view of what is being attempted by E. A. Milne in his theory of "kinematical relativity." Nevertheless we shall see that the "model" method is not avoided by him, but that he produces what have to be regarded as *physical* models of the universe, in the sense now to be explained.

[18] Clerk Maxwell, *Scientific Papers*, Vol. I (1890), 156.

KINEMATICAL RELATIVITY[19]

Milne's theory may be stated from the point of view which regards his postulates as hypotheses about the universe of experience. But this presentation has proved in practice to provoke mistaken criticism that it starts from the assumption that the postulates are known to be empirically "true," when clearly they have never been submitted to experimental test. So we may alternatively regard his postulates as *defining* a purely theoretical model universe, i.e. Milne virtually says, "Suppose we imagine a universe, in which observers can make certain types of observation[20] and such that the postulates about the equivalence[21] of fundamental observers and the cosmological principle[22] are satisfied. What further properties can we deduce for such a universe, meaning properties discoverable by the observers making the types of observation postulated?" Milne's theory is then a purely deductive theory based on physical postulates, strictly in the sense that *in his model universe* the equivalence of observers and the cosmological principle could be arrived at by the observers as empirical "laws," and all the other properties are deduced from these without further assumptions. The final step is to compare the model universe so constructed with the universe of experience. We shall examine below what is learned by the comparison.

This attitude towards Milne's work appears to be consonant with his own,[23] though it has at this stage nothing to do with his attitude towards the principle of induction. We shall find it relevant to notice that it represents the same general attitude as that which he adopts, in an entirely different field of investigation, towards his work on *stellar structure*.[24] He has there always insisted on the desirability of constructing theoretical stellar models based on well-defined initial assumptions and with no

[19] E. A. Milne, *W. S.* and subsequent papers by Milne, A. G. Walker, and G. J. Whitrow, in *Proc. Roy. Soc.*, *Proc. London Math. Soc.*, and elsewhere.

[20] W. S., § 15.

[21] W. S., § 11.

[22] W. S., Ch. III.

[23] E. A. Milne, *Proc. Roy. Soc. A. 158* (1937), 324; see 325–6.

[24] See the series of papers from 1929 onwards in *Monthly Notices of the Roy. Astron. Soc.*

subsequent introduction of empirical features. The aim is to accumulate a variety of models deduced from different sets of what are regarded *a priori* as "reasonable" physical assumptions. Then one hopes that it will prove possible to select one model which reproduces the main characteristics of a certain class of star to a sufficiently close approximation, and to an appreciably better approximation than does any other of the models. In that case one would conclude that this class of star is actually constructed approximately in the same way as the selected model. Of course it is implied that this conclusion would be "true" only with a certain degree of probability.

To return to kinematical relativity we now note that it does reproduce many results in good agreement with observation. Firstly, it yields the Lorentz transformation for passing from the coordinates assigned by one observer to those assigned by another, so that certain of the observational tests of the special theory of relativity can be applied to Milne's theory also. Then it yields a distribution in space and velocity of the spiral nebulae which may be said to agree with observation to a first approximation, this being all that can apparently be legitimately claimed for any existing theory in this connection. It reproduces phenomena which can be matched with the behaviour of the cosmic cloud and cosmic rays in the actual universe. It yields the acceleration of a free particle in such a way that a classical interpretation would yield something very like the inverse square law of gravitation. Finally it receives indirect support from observation through the fact that it provides two time-scales and does not, like most other theories, reach irreconcilable conclusions when it attempts to estimate the "age of the universe" from various data.

Some of the predictions of the theory are not at present susceptible of very exact quantitative tests. But suppose for the moment that when the necessary observations are made the predictions turn out to be in good agreement with them. What could we then conclude about the theory? The immediate conclusion is that the final mathematical results of the theory, when interpreted according to the rules of the theory, are in good agreement with observation (which is only saying the same thing over

again). But we wish to see further how far the agreement allows us to conclude that the actual universe is constructed in the same way as Milne's model. We again cannot test this directly. For in Milne's theory his fundamental observers are associated one with each spiral nebula regarded as a "particle." Since we have at present no communication with observers on other nebulae we cannot test the postulated equivalence of observers or the cosmological principle directly. So once more they become hypotheses to be tested by their consequences, and as can be seen by a closer examination of the theory, it is the consequences of the postulates *taken together* which are being examined. Also there are certain assumptions involved in Milne's prescription of possible observations, which are not *a priori* necessarily true of the actual universe, and also cannot be directly tested.[25]

Once again, as in the previous theories examined, we have the feature of hypotheses which are tested only by the final consequences of the theory. In the present case Milne's hypotheses appear to be the simplest from which a model universe could be derived. But they are not intuitive or inevitable. In fact it is not difficult to see how other models might be constructed. For example, the writer has pointed out (loc. cit) that it is possible to have a model in which Milne's cosmological principle is satisfied without having his first type of equivalence satisfied for every pair of observers. He has also pointed out what would be involved in a relaxation of the restrictions on the allowable observations. Indeed A. G. Walker has given a treatment of this problem and describes his work as obtaining "what I believe to be the general solution based upon these principles ("the same general principles laid down by Milne") and certain other hypotheses," and proceeds, "and it will be seen that this solution includes as special cases all of the systems provided by general relativity, as well as Milne's system." This being so, we reach a position analagous to that which we have seen has to be adopted in regard to Milne's stellar models; we now have various model

[25] These are discussed more fully by W. H. McCrea, *Proc. Edinburgh Math. Soc.* (1938), in press.

universes and have to see if we can select one which reproduces the observable properties of the actual universe better than do the rest.

This leads one to venture to criticise Milne's contention that he has put the laws of dynamics on a new status as "logical inferences from axiomatic definitions" so that it is "no longer necessary to appeal, as a warrant for a belief in the 'truth' of these laws of nature, to the so-called 'principle of induction' ".[26] Our brief discussion makes it clear that the ultimate reason for belief in the "truth" of Newton's, or Einstein's, or Milne's laws of dynamics must be the same, namely agreement of the consequences of one theory or another, as a whole, with observation. The only difference is that Newton's theory makes the "laws" themselves the postulates (or hypotheses) of the theory, while Einstein's and Milne's theories deduce them from other postulates. In none of the cases are the "laws" either direct inductions from observation, or consequences of inescapable postulates.

EDDINGTON'S THEORY

Before passing to a general discussion, we should notice certain aspects of Eddington's recent work.[27] This is mainly concerned with the relations between relativity and quantum theory, and so lies for the most part outside the present discussion. It does however to some extent involve a re-casting of space-time theory, though as far as this alone goes it does not present final results divergent from those of general relativity. But the general conclusion of the work is new and, at least at first sight, revolutionary. This conclusion is that certain general laws of Nature can be reached by *a priori* methods. Indeed, with Eddington's meaning of a law of Nature, it appears that he concludes that they must be so reached, that they are in fact consequences purely of our methods of thought and methods of making measurement. One cannot refrain from giving the passage in Eddington's book (p. 327), which has so often been quoted since its

[26] Milne, *Proc. Roy. Soc.*, *loc. cit.*, p. 326.

[27] A. S. Eddington, *Relativity Theory of Protons and Electrons* (1936). See also *Nature*, *139* (1937), 1000–1001.

appearance, recalling however that Eddington describes it (*Nature loc. cit*) as "not the basis but the unexpected conclusion of the investigation." It is, "An intelligence, unacquainted with our universe, but acquainted with the system of thought by which the human mind interprets to itself the content of its sensory experience, should be able to attain all the knowledge of physics that we have gained by experiment. He would not deduce the particular events and objects of our experience, but he would deduce the generalisations we have based on them. For example, he would infer the existence and properties of radium, but not the dimensions of the earth."

Two comments are relevant. First, this conclusion seems to assume that the generalisations we have based on the events and objects of our experience are exact, and final in regard to our system of thought, for the mathematical deductions, which it is now sought to identify with them, are exact, and are claimed as logical consequences of our system of thought, and therefore presumably final in regard to it. "Generalisations from experience are not, however, made with certainty,"[28] so that it is difficult to attach a precise meaning to this identification.

Second, the conclusion assumes that our system of thought is known exactly. This raises a difficult point, for Eddington might assert that all that he requires to assume about it is immediately known to the mind, and would be agreed to by all competent human minds, just as they would agree about a proof in pure mathematics. On the other hand there may be a hypothetical element in it. If so, an exponent of Eddington's theory would presumably claim for its support that, when he starts with the hypothesis that the human mind works in the way supposed by Eddington, then he reaches conclusions in agreement with generalisations from experience, about which there is general accord amongst scientists. If this latter attitude were adopted, then Eddington's theory would share with the others we are examining the same general characteristic of starting from certain hypotheses whose acceptability would depend upon the agreement of

[28] H. Jeffreys, *Nature*, *141* (1938), 672–6, 716–719, which see for a recent and concise discussion of this point.

their ultimate consequences with observation. The essential difference would then lie in the nature of the hypotheses themselves. To contrast Eddington's theory with Milne's, for example, instead of postulating something about specific observations that can be made by observers, as in the latter, we see that the former would postulate something about the minds of the observers and the meaning they attach to observation in general.

SIGNIFICANCE OF THE THEORIES

The position we have now reached is that all the theories we have examined here, with the possible exception of Eddington's, have in broad outline the same structure. They start with hypotheses from which are deduced certain mathematical relations which, with certain rules of interpretation, finally yield relations between observable quantities. The hypotheses consist of statements which are not known *a priori* to express physical "truths", and which in general cannot be tested directly and severally by comparison with experience. The final relations yield statements of the sort that, if certain things have been observed, then certain other things are necessarily observable (for example, that if the planet Mercury has been observed in a certain sequence of positions at a certain set of epochs, then if it be observed at such and such an epoch it will be found to be in such and such a position; or again, that if certain spiral nebulae are found to have apparent brightnesses in certain range, then if their spectra be observed they will be found to show redward shifts lying within such and such a range). It is convenient to call these relations the *predictions* of the theories.

We regard as the testing of a theory the comparison of its predictions with experience. But the meaning of this is not very simple. For in the first place the theory in general makes potentially an infinite set of predictions while, of course, we test only a finite number. But even in any particular case the theoretical prediction is in general exact, while an exact measurement has no meaning. The proper discussion of this aspect of the problem cannot be given without introducing notions of probability.[29]

[29] Cf. Jeffreys, *Nature loc. cit.*

However, mathematical physicists have a generally accepted working conception of what is involved in the comparison of theory and observation, and it will serve our present purpose to keep to this level of ideas.

Physics is concerned, broadly speaking, with two types of problem, problems of existence and problems of behaviour. The distinction would require considerable analysis to render it precise, but it may be roughly explained as follows: The mathematical relations yielded by a theory concern the behaviour of entities whose existence is implied by rules of interpretation which also form part of the theory. To take an example, Clerk Maxwell pointed out that, "if we knew nothing more than is expressed in the mathematical formulae, there would be nothing to distinguish between the one set of phenomena and the other", when he was commenting on the identity of the mathematical laws of "the uniform motion of heat in homogeneous media" and those of "attractions varying inversely as the square of the distance".[30] In such a case different rules of interpretation apply for the two sets of phenomena. Such rules presuppose the existence of certain physical entities; in this example, one set presupposes the existence of *heat*, etc., and the other of *mass*, etc. Additional theory might "explain" *heat* in terms of the motions of *atoms* and *molecules*, but only by presupposing the existence of these entities, and so on.

Now until quite recently it was generally assumed that we should always have to presuppose some "given" entities. Practically the only suggestion that we might not have to do so comes from Eddington's new work. We shall not presume to enter further upon this field, but shall in the remainder of this paper confine attention to the theories of time and space and mechanics already treated. It is clear that none of them represents an attack on the fundamental problem of existence of physical entities, but are concerned only with the problem of behaviour.

One now wishes to put forward the view that all that is directly significant in relation to physics in any of these theories is the set of mathematical relations to which it leads, together with

[30] Clerk Maxwell, *Scientific Papers*, I (1890), 157.

the set of of rules for interpreting them so as to derive relations between observable quantities. There is, of course, nothing new in this. However, the writings of some mathematical physicists suggest that they would not subscribe to it, but that they would attach significance to the idea that one theory gives a better "understanding" of phenomena than does another, apart from the detailed agreement of the predictions with observation, and also to the idea that one theory starts from a more "reasonable" set of hypotheses than another. For example, they would probably argue that general relativity gives a more philosophically satisfactory treatment of time and space than does classical theory, even were its predictions in no closer agreement with observation. The present point of view would deny significance to such a contention.

The only sort of physical problem that faces these theories is this: If we have made certain observations (pointer-readings) and got certain results, can we predict the results of another set of observations (pointer-readings) which we know, or believe, we can make?[31] A given theory either can or cannot give an answer. If it can give an answer it is because the data supplied by the first set of observations are sufficient for us to apply the mathematical relations of the theory and its rules of interpretation to make the required predictions. We then have perfectly definite criteria for deciding between the merits of different theories. If in a specific problem of the sort stated one theory makes a prediction which turns out to be in closer agreement with observation than does a second theory, we say it is "better" in relation to this particular problem. But we do not usually judge by isolated problems but by a class of problems associated with a class of physical phenomena. However the same criterion of "goodness" of theories applies then also, as soon as we have laid down tests of "goodness of fit" for the whole set of predictions involved. Further, we may ask what data are required by a given theory in order to make a particular prediction or class of predictions. If one theory requires fewer data than another to

[31] In certain cases the predictions may refer to the statistical results of a set of observations.

make a prediction of the same degree of accuracy, we say that it is a "better" theory in relation to this criterion. Or again we may ask what classes of prediction can be made by a given theory from a certain set of data. If one theory is able to make more classes of prediction than another, to the same degree of accuracy, then it is a "better" theory on this count.

This is all quite elementary, but it represents everything that physics is concerned with in the theories. In short, physics must regard increased understanding of the physical world as synonymous with increased skill in prediction, for that is all we can test by physical means. In other words the mathematical relations, suitably interpreted, give a description, rather than an explanation, of the working of the physical world. They turn out to be essentially the same character as "empirical laws", only they are generally reached by a rather more round-about route than the relations which commonly go under this name, and not by the relatively simple process of fitting a curve to a certain number of observations.

Continued practice at making predictions according to a particular theory gives one a so-called intuitive ability to foresee in a qualitative way, without making detailed calculations, what the solutions of certain classes of problems, according to this theory, will be like. This is identically the same ability as that which a pure mathematician develops when, by increasing familiarity with some particular branch of his subject, he becomes able to see how a certain problem will "go" without carrying through the argument step by step. But some physicists appear to attach added significance to this stage of thought. For instance, writing about quantum mechanics, C. G. Darwin says,[32] "the new discoveries. . . .are still partly covered with the scaffolding of mathematical formalism. But I hope to show that the time has already come when it is possible to free the structure from a good deal of this scaffolding and so to gain something of an intuitive view of what the world is really like." The attitude now being advocated would lead one to say that the world is "really like" the description given by the "mathematical formalism", and

[32] C. G. Darwin, *New Conceptions of Matter* (1931), 4.

that the "intuitive view" represents merely a more sketchy treatment of the mathematics. Moreover, by turning the emphasis the other way round we run the risk of creating illusory physical problems, as in the case we have already noted in regard to "action at a distance."

It is now easy to see, without going into details, what would be the general results of a comparison, according to the present criteria, of the merits of the theories under discussion. The final mathematical relations of Robb's theory are the same as certain of those of special relativity (it does not deal with mechanics or electromagnetism). The physical phenomena to which it applies are certain ones connected with the propagation of light, and in regard to these phenomena it is a better theory than Newtonian theory, since it is known that special relativity gives results in closer agreement with observation than does the latter. However, in other respects it is not such a good theory in that it cannot make predictions in regard to such a wide field of phenomena. General relativity is better than both the previous theories on all counts. Milne's theory has not been submitted to detailed tests, so it is not possible to make a general assertion that it is better or worse than the others. However there is already indication that it may prove better in the treatment of time through its use of two time-scales.

THE RÔLE OF HYPOTHESES

In our brief description of the several theories we were led to emphasise their common feature of the presence of hypotheses. But it now appears, from the standpoint we are urging, that the hypotheses have no significance in the final physical theories. Here we mean the hypotheses from which the mathematical relations are deduced, for, of course, if we wish we may re-formulate each theory by saying that it makes the hypothesis that the behaviour of the physical world is described by a certain set of mathematical relations suitably interpreted, and proceeds to test *this* hypothesis. The latter would be strictly true, while it is not strictly true that the comparison with observation tests the initial hypotheses from which the mathematical relations are de-

duced. For more than one set of initial hypotheses may lead to the same testable mathematical relations. For instance, the mathematical relations of special relativity have from time to time been deduced from quite different sets of hypotheses.[33]

Nevertheless it would be absurd to contend that the initial hypothesis are unimportant. On the contrary, it has been part of our purpose to show that the successive space-time theories have followed a natural sequence of hypotheses, and to show their fruitfulness in yielding results. But what we do venture to contend is that they belong to a non-physical and non-logical part of the theories. They are non-physical for the reasons already explained, and are non-logical in the sense that they cannot be arrived at by any strictly logical process. In the same way there is no set of rules of logic which can be applied to yield the axioms of a mathematical science.

In sketching the development of the specific theories we have actually suggested ways in which one may be led to try hypotheses of a certain type. In the case of Einstein's field equations we have seen how a suggestion comes from a certain analogy of mathematical form with the equations of a previous theory. In this activity of forming hypotheses the "intuitive view" alluded to above seems also to play a part. For when we attain to an intuitive view of the working of one particular theory we can sometimes guess at an intuitive view of the working of a "better" theory, and then seek by trial and error to modify the hypotheses of the first theory, or replace them by others, so as to get a precise theory which in turn will lead to something like the surmised new intuitive view. Another common method is to take results suggested by one theory, and make them the hypotheses of a new theory. An illustration might be supplied by Milne's hypothesis of the cosmological principle. It could have been reached in the following way, though to what extent this was in Milne's mind it is impossible to say: The simplest solutions of the equations of general relativity which represent entire universes correspond to homogeneous universes. But it was found that these simple solutions apparently reproduced the large

[33] G. T. Whitrow, *Quart. Journ. of Math.* (*Oxford Ser.*), *4* (1933), 161–172.

scale properties of the actual universe remarkably well. So if we are going to construct a new theory based on hypotheses about observers, it becomes quite natural to make it one of the hypotheses that there is in the universe a set of fundamental observers having the property that each member bears the same relation to the whole set as does any other. Thus the homogeneity suggested by certain applications of general relativity reappears as a hypothesis of kinematical relativity.

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Vagueness and Logic

BY

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AS IS rather generally admitted today, the terms of our language in scientific as well as in everyday use, are not completely precise, but exhibit a more or less high degree of vagueness. It is the purpose of this paper to examine the consequences of this circumstance for a series of questions which belong to the field of logic. First of all, the meaning and the logical status of the concept of vagueness will be analyzed; then we will try to find out whether logical terms are free from vagueness, and whether vagueness has an influence upon the validity of the customary principles of logic; finally, the possibilities of diminishing the vagueness of scientific concepts by suitable logical devices will be briefly dealt with.

As starting point for the subsequent considerations we choose the clear and stimulating analysis of the concept of vagueness which has recently been carried out by Max Black ({1}),[1] and which has suggested the considerations of this paper.

Distinguishing vagueness from generality and ambiguity, Mr. Black characterizes the vagueness of a symbol by "the existence of objects concerning which it is intrinsically impossible to say either that the symbol in question does, or does not, apply" ({1}, p. 430). "Thus a word's vagueness is usually indicated . . . by some statement that situations are conceivable in which its application is 'doubtful' or 'ill-defined', in which 'nobody would

[1] Numbers in curved brackets refer to the bibliography at the end of this paper.

know how to use it' . . . " ({1}, p. 431). Therefore, Mr. Black states, "the vagueness of a word involves variations in its application by the users of the language in which it occurs" ({1}, p. 442) This observation leads him to a very interesting proposal for the numerical determination of the vagueness of a term by means of what he calls the *consistency of application* C(T, x) of a term T to some object x. Let, for example, the symbol be the word "plant" and the object one of those deep-sea organisms which belong to the border zone between plants and animals. If several observers are asked whether the term "plant" does or does not apply to that object, there will be a certain number m of affirmative answers and a certain number n of negative ones. The consistency of application of the term "plant" to that object is then defined by Mr. Black as the limit of the ratio $\frac{m}{n}$, when the group of observers is more and more extended, and the number of decisions made by its members is indefinitely increased.—If several objects are given to each of which a certain vague symbol T may be applied, then the objects may be put into a linear order in which the consistency of application of T steadily decreases; the series thus obtained will begin with those objects to which all or most observers apply the term T (i.e. for which C(T, x) has a large value), and it will end with those objects to which all or most observers apply the negation of T (for these objects, C(T, x) is small); the middle section will contain the $\gg\ll$ borderline cases (with values of C(T, x) lying in the neighbourhood of 1). If each of the considered objects is represented by a point on a horizontal axis, and the corresponding value of C(T, x) is plotted vertically above it, a curve is obtained which Mr. Black calls the *consistency profile for the application of the vague symbol T* to the given series of objects. The steepness of this curve in its middle section is then taken as exhibiting the vagueness of the symbol to the given series of objects: the steeper the drop, the smaller the number of doubtful cases, and therefore the smaller the vagueness (the greater the precision) of the symbol. An adequate mathematical definition of this steepeness would therefore, in Mr. Black's view, also determine a definition of vagueness.

For the subsequent considerations, we shall make the general ideas of Mr. Black's interpretation of vagueness our own, without adopting, however, each of the technical details of his analysis.

There is, in fact, a gap in the above determination of vagueness[2]: The given definition amounts to measuring the vagueness of a symbol by means of the steepness of its consistency profile; and Mr. Black holds that a numerical measure of that steepness can easily be defined. This is, however, not the case, for such a definition would presuppose the existence of a metrical scale on both the horizontal and the vertical axis of the coordinate system. This condition is not fulfilled for the horizontal axis: the objects of the series have simply been put into what may be called a topological order; the principle of the arrangement being that each object precedes all those for which the consistency of application of T has a smaller value. But there is no additional criterion stipulating under what conditions two objects in the linear arrangement are to be represented as equidistant with two other objects; in other words: no metrical order is defined on the horizontal axis. For this reason, the concept of steepness is not applicable to the consistency profile and cannot serve to introduce a measure of vagueness. This point does, however, not affect the basic idea of Mr. Black's conception of vagueness. It is, in fact, possible to give a modified definition of vagueness which corresponds to Mr. Black's general idea and is free from the difficulty which has just been pointed out. This may be done as follows: We first introduce a slight modification in the definition of the consistency of application of a term T to an object x, by defining C (T, x) as the (limit of the) ratio $\frac{m}{m+n}$, where m and n are the numbers explained before. As a consequence, C is now restricted to the values 0 to 1 (both inclusive), and the doubtful cases are those for which C(T, x) lies in the neighbourhood of $\frac{1}{2}$.— Now let us suppose that we are given a vague symbol T and a series S of objects $x_1, x_2, \ldots, x_n$, in which each object x determines a certain value C(T, x). Then the vagueness of T — as applied to the elements of S — will be so much the greater, the more objects there are for which C (T, x) lies in the neighbourhood of $\frac{1}{2}$. Or, to put it in a different way: the more the values of C (T, x) in the series differ from $\frac{1}{2}$, the greater

[2] The following remarks in small print may be left out by readers who are not interested in the details of a mathematical definition of vagueness in the sense of the general ideas outlined before.

the precision of T as applied to the series. This idea may be expressed by the following definition for the precision of T as applied to the series S:

$$\mathrm{pr} = \frac{4}{n} \sum_{\nu=1}^{n} [C(T, x_\nu) - \tfrac{1}{2}]^2.$$

The vagueness of S as applied to the same series may be defined as the complementary value:

$$\mathrm{vg} = 1 - \mathrm{pr}$$

The definitions have been chosen in such a way that all possible values of pr and vg lie between 0 and 1, both included.

2. To what scientific discipline does the study of vagueness belong? No doubt, it is a task incumbent on a general theory of signs, or, to employ the term used by C. W. Morris, on *semiotic*. As Professor Morris expounds in detail,[3] semiosis—i.e., the process in which something functions as a sign—involves at least three correlates, namely the sign, the subject matter it refers to, and those who use the sign or respond to it. There are some important branches of semiotic which deal only with one or two of these correlates: syntax, for example, is exclusively concerned with the formal properties and relations of linguistic expressions; and semantics[4] deals with the relation between symbols and their "*designata*", i.e. the objects, properties, relations, or whatever else the symbols designate. But there are other parts of semiotic theory which have to take into account all the correlates involved in semiosis; and the study of vagueness is an interesting example of this type: "Vagueness" does not simply designate a formal property or relation of the symbols occurring in a language, nor a semantical relation between symbols and their designata; "vagueness" is what Prof. Morris calls[5] *a strictly semiotical term;* its determination requires reference to the symbols, their users, and their designata: As may be seen from the above general discussion a complete statement about the

[3] Cf. Morris {1}.

[4] The concepts "syntax" and "semantics" will be explained in what follows. For fuller details on syntax, cf. Carnap {1}, {2}, on semantics Kokoszynska {1}, Tarski {1}; on syntax and semantics Carnap {4}, Morris {1}.

[5] Morris {1}, p. 8.

vagueness of a symbol is of the following type: "The vagueness of the term T, as applied, by the group G of persons, to the elements of the series S of objects, is v". Thus, *vagueness* is what may be called *a three-place semiotic relation which may assume different degrees* (the values of v), or, in more technical terms: *a strictly semiotic function of three arguments* (T, G, S being its arguments and v its value).

3. Statements about the degree of vagueness with which a term is used by a certain group of persons, are obviously empirical in character; they refer to the speaking or writing behaviour of certain individuals; their establishment requires empirical investigation. This behavioristic character of vagueness will perhaps be rendered more explicit by the following example which does not establish a mere analogy, but another genuine case of vagueness outlined in 1.

Let us consider a set of balances of that automatic type which give out cards indicating the weight of the load. These balances may be regarded as a group of individuals speaking a very simple common language. The terms of this language are vague, just like those occurring in any historical language. Their vagueness will manifest itself in two ways: First, if the same load is weighed several times by means of the same balance, the results obtained may nevertheless not all coincide, provided the smallest units which the balance distinguishes in its printed statements are not too large. These ≫random variations≪ of the indications obtained from one and the same indicator consitute a well known feature which all measuring instruments have in common.— Second, if the same load is consecutively weighed with the help of each of the balances, the results obtained will again not all coincide. Some of the ≫speakers≪ of that group will perhaps ≫apply to the given load the term "60 kgs"≪, others, by yielding a different statement, will ≫assert that that term does not apply≪. Thus, a certain number m of positive and a certain number n of negative ≫answers to the question as to whether the term "60 kg" applies to that load≪ will be obtained, and the consistency of application of that term to the load in question,

within the considered group of speakers, may be defined in agreement with Mr. Black's proposal.—Again, the deviations of the results obtained in determining the same magnitude by means of several indicators of the same type, is common to all measuring instruments, including living organisms.

4. As a consequence of the preceding considerations, it may be noted that the phrase "the degree of vagueness of a symbol" is obviously an elliptic expression (which, however, is often unambiguous and convenient to be used); for vagueness, as has been pointed out in section 2, is not a property of a symbol, but a semiotic function whose definition involves reference to the symbol, its subject matter, and its users. An analogous remark applies to Mr. Black's statement that "vagueness is clearly an objective feature of the series to which the vague symbol is applied" ({1}, p. 440/441). How would this statement run in a complete formulation? What it is to express may, as it seems to me, be described thus: The vagueness of a symbol T in the series S is defined by reference to the decisions made by a group G of observers. However, its value will remain approximately the same, when the group of observers originally referred to, is replaced by another. In other words, the degree of vagueness of T as applied to S is, within certain limits, independent of the special group of observers which is used in its determination.

We need not discuss the validity of this assertion here; within certain limits, it seems in fact to be confirmed by what Mr. Black calls the success with which vague symbols are used. However, the following point ought to be noticed:

In so far as that statement holds, the degree of vagueness of a symbol may be defined as a function of only two (instead of three) arguments, namely T and G; but in the definition, reference would have to be made to ≫some group of users of the symbol≪; this reference would be essential, it could not be dispensed with; therefore, vagueness would even then be a strictly semiotic function, not a semantical one.[6]—As this example illustrates, a

[6] For the question as to whether it is not possible to introduce a purely semantical parallel concept to "vagueness" cf. section 7.

strictly semiotic relation or function need not necessarily have three members corresponding to the three correlates of semiosis; it is sufficient for its strictly semiotic character that in its definition, reference has to be made to symbols, their subject-matter, and their users.

5. Is vagueness a peculiarity exhibited by a special class of terms only? It is perhaps that only descriptive symbols may be vague, whereas logical symbols[7] are always precise? To answer this question, the analysis of vagueness must be advanced a little further.

So far, we have simply said that the vagueness of a symbol appears in the fact that different observers will not always give the same answer to the question as to whether the symbol does or does not apply to a given case; in other words, the vagueness of a symbol has been characterized by certain variations occurring in its use. Now, we have to distinguish these variations from others which would not be considered as symptomatic for vagueness. Let us suppose that two persons observe the colour of a certain object under the same external conditions, and that one of them asserts the object to be yellow whereas the other denies this. Then this variation in the use of the term "yellow" may be due to differences in the observers' perceptual apparatuses (including, possibly, auxiliary devices such as optical instruments); or one of the observers may consciously have made an assertion which is not in agreement with the result of his observation. Variations in the use of a symbol which are caused by factors of these kinds will obviously not be considered as symptoms of vagueness.

On the other hand, it may happen (and really does happen very frequently) that two observers differ in their way of applying a certain term, say "yellow," though no factor of the kind just mentioned is realized. In fact, it may be that on account of the speaking habits of the persons in his environment, the first observer has been trained, from his childhood, to use the

[7] These terms are illustrated in what follows. For a precise account, cf. Carnap {2} and {4}.

term "yellow" in a rather liberal way so as to cover, say, certain shades which the other observer has been trained to term "green"; besides, neither of the observers—just like any other person learning his mother tongue—will have been taught to draw a sharp border-line between yellow and not-yellow shades; and these two features are characteristic for what is called the vagueness of the term "yellow". Thus, the vagueness of a symbol consists in the existence of a speaking habit among its users which involves the occurrence of those variations in the application of the symbol which have been illustrated by the last example, and which are not due to different perceptual abilities or to conscious misuse of the term.

It is obvious that this sort of variation in use will be exhibited, first of all, by all descriptive terms, i.e. by those terms which designate empirical objects, properties, relations, and the like; such as "Mt. Everest", "yellow", "similar in appearance", etc.: The appropriate way of using these terms is mostly learnt by means of ≫ostensive definitions≪; thus, for instance, a child is taught to what objects it is to apply the term "yellow" by pointing at various yellow objects—a method which naturally leads to variations in the use of that term even with persons who have the same abilities of colour distinction, and who seriously try to use the term in the ≫correct≪ way.—And as to those descriptive terms which are introduced by some sort or other of theoretical definition, they inherit the vagueness of the primitive terms on which the definitions are based.

From quite similar considerations it may be seen that none of the logical terms occurring in historical languages—such as "not", "and", "if—then", "every", "there is", etc. in English—is absolutely precise either: People learn the usage of the logical terms of their mother language by means of examples and illustrative comments which do not prevent the occurrence of variations in their application; the long discussions about ≫the meaning of existential sentences≪, for example, exhibit such variations in the use of the term "there is".

Thus we come to the result that no term of any interpreted language is definitely free from vagueness. (We have to exclude

from this statement those artificial languages which have not been given any interpretation; for with respect to their terms, the question of vagueness does not even arise since one of the correlates of the semiotic relation of vagueness is missing, namely the designata of the linguistic terms.)

6. Do the principles of logic lose their general validity when vague symbols are involved?

Mr. Black raises this interesting question and answers it in the affirmative. His discussion of the problem is based on the assertion that with respect to a vague symbol, there are certain "doubtful objects" or "border-line cases" concerning which "it is intrinsically impossible to say either that the symbol in question does, or does not, apply". The main point in Mr. Black's argument ({1}, p. 435) may briefly be stated thus: Let "L" be a vague one-place predicate, and b an object which constitutes a doubtful case with respect to "L". Then b has to be included in the class of all objects which have the property L; since, as a doubtful object, it is not positively excluded from that class. Analogously, b has also to be included in the class of all objects which have the property not-L. This is clearly "incompatible with the usual definition of negation, and thus indirectly incompatible with the strict application of logical principles "({1}, p. 435). To overcome these difficulties, Mr. Black suggests introducing a more complex logical symbolism, replacing each vague one-place propositional function "L(x)" by a two-place one "L(x, C)", whose second argument indicates the consistency of application (as defined above in section 2) of "L" to x. "L(x, C)" is to be read "L applies to x with consistency C" ({1}, p. 451). If, now, the consistency of application of "L" to x is C, then, by Mr. Black's definition, the consistency of application of "$\sim$L" to x is $\frac{1}{C}$: "Thus the principle of excluded middle is replaced by the operation which permits the transformation of L(x, C) into $\sim L\left(x, \frac{1}{C}\right)$"({1}, p. 452).—

For a discussion of the questions which arise in this context,

it is important to distinguish *two different ways of dealing scientifically with a given language*, and in particular with the ≫laws≪ of its logic.

The first way may be called *the behavioristic approach:* The given language—say, that of a newly discovered tribe in Borneo—is examined as an empirical phenomenon, namely as a part of the total behavior of its users. An investigation of this type would aim at determining the different expressions of the language and the modes of their combination; further, the subject matter which they serve to refer to, the conditions under which they are employed, and the psychological effects by which their use is accompanied in the users and in the persons to whom the considered utterances are addressed. The results thus arrived at can be expressed in empirical statements. They are comparable with the description which an observer who does not know the rules of chess might give of the playing behavior of people playing chess.

Second, the attempt may be made to abstract, from the empirical evidence thus collected, a *theoretical linguistic system* which is *governed by precise rules of syntax and semantics*. The syntax determines the terms of the language, the rules according to which they are combined into sentences, and the principles of logical inference. The semantics determines the meanings of the linguistic expressions. This may be done, for example, by defining the *relation of designation* for the given language; in other words, by indicating for each of its words its designatum, i.e. the object, property, relation or whatever else it designates, and by setting up rules which determine the meaning of the complex expressions, mainly of the sentences, in terms of the meanings of the words of which they are composed.

In abstracting such a theoretical linguistic system from the empirical data obtained in the behavioristic study of a language, one has to omit any reference to the users of the language, to their psychological reactions connected with the use of the terms, etc.; in other words, one has to disregard those features of the language which form the subject matter of what Prof. Morris calls the pragmatical rules of the language.[8] This procedure is

[8] Morris {1}, p. 35.

comparable, in the example of chess, with an attempt of the observer at abstracting a theoretical system of rules of chess from the evidence established by behavioristic observation. In doing so, the observer would dismiss many features as ≫inessential≪, such as the fact that before moving a chessman, a player will often frown thoughtfully, that a player, when pronouncing the words "check-mate", displays in general more signs of pleasure than his partner, etc.

In the case of a language as well as in that of a game, the theoretical system thus obtained is an ≫abstraction≪. It is no longer properly descriptive of the way in which the language is actually used or the game is actually played by a certain group of people. Its logical character is rather that of a set of empirical statements. Though, of course, the choice of those rules and definitions is directed by the empirical evidence found in the behavioristic study, and though the theoretical system is set up in such a way as to correspond as far as possible to the results of those studies, it is no longer properly descriptive. The rules of a theoretical system of chess will sometimes be violated by people playing chess; and the rules of the theoretical syntax and grammar of an interpreted language, say of French, will often be violated by the users of that language.

Now, these rules which are usually called the logical principles of a language, form a part of the theoretical system of syntax and semantics which is established by the abstractive process which has just been discussed: the logic of a language is determined by its syntax and semantics.[9] Therefore, the last statement applies in particular to the logical principles of a language: None of them is strictly descriptive of the speaking behavior of its users; each of them will sometimes be violated in the actual use of the language, even if it is as common as the principles of excluded middle and of excluded contradiction.

The vagueness of all linguistic terms favours the occurrence of such violations (though it is not their only possible source). In fact, if, e.g., some observers apply to a certain object the term

[9] For the connections between syntax and logic, see Carnap {1}, {2}; for semantics and logic: Tarski {1}, Carnap {4}.

"yellow", others the term "not yellow", and if all the statements thus established are included in one common system of assertions, this system violates the principle of contradiction.

However, the last remark cannot be interpreted as proving an incompatibility between the vagueness of the terms of a language and the validity of logical principles. In fact, as has been shown above, the question of logical principles arises, strictly speaking, only on the abstract level where language is dealt with as a theoretical semantico-syntactical system; the questions concerning vagueness, on the other hand, refer to language as a form of behaviour, as a system of linguistic utterances of a group of speakers; and on this behavioristic level, any form of logic, the customary as well as any new one, will sometimes be violated by the actual speaking utterances of the users of the language under consideration.

7. But is it not perhaps possible to reformulate the question as to the influence of vagueness on the validity of logic in such a way that it refers to languages in the sense of theoretical linguistic systems?

An affirmative answer to the question would obviously presuppose that something analogous to the original concept of vagueness, which we found to be strictly semiotical, can be introduced on the level of purely syntactical and semantical investigation.

The supposition that this might be possible is favoured by the fact that, for some strictly semiotic concepts, it is indeed possible to establish purely semantical parallel-concepts. Thus, for instance, the purely semantical two-place propositional function "the French term x designates the property y" may be considered as a parallel concept or analogue with respect to the strictly semiotical three-place propositional function "the group z of speakers designates by the French term x the property y".

In order to introduce a semantical analogue of "vagueness", it seems to be necessary to express the idea that a term may apply to different objects in different degrees. The totality of the different degrees corresponding to the different objects to

which the term may be applied, might then serve to determine the vagueness of the term in question, according to the considerations developed in section 1.—We have, therefore, to ascertain whether the semantical relation which obtains between a term and the subject matter it applies to, is amenable to gradation. Now, what has been called here "applying" has (at least) two different meanings. To illustrate them, let us take French as the linguistic system under consideration. If a term x is not a predicate like "jaune", "à côté de", "entre", etc., or a functor[10] like "température", but a proper name, then "x applies to y" simply means "x designates y", such as for instance in the sentence "(The name) 'Londres' applies to (the city) London".

If, however, the term in question is a predicate or a functor, the relation of application coincides with what is usually called denotation;[11] it is, then, analyzable into the product of two simpler relations; the first being the relation of designation which has just been considered, and the second the converse of the relation ϵ which holds between an object and any property which the object possesses. Thus, e.g., the statement that the term "couteux" applies to the koh-i-noor may be analyzed thus: "couteux" designates a certain property of objects (namely, the property of being expensive), and that property is owned by the koh-i-noor.

Thus, the relation of application has turned out to coincide with that of designation or to be analyzable into a product with designation as one of its factors. Therefore, in order to introduce a gradable relation of application, it appears to be necessary to gradate the relation of designation. Such a procedure, however, —though not logically impossible—would lead to rather extraordinary consequences: If the semantics of a language is determined by means of a relation of designation which admits of degrees, that language cannot be translated into English. Let us first consider a linguistic system whose semantics is based on the customary (not gradable) relation of designation. The semantics of French, for instance, might contain the indication that the noun "le soleil" designates the sun, the adjective

[10] Cf. Carnap {2}, p. 14.
[11] Cf., e.g., Morris {1}, p. 5.

"chaud" the class of hot things, or, what amounts to the same, the property of being hot; further it may contain rules to the effect that a sentence of the form ".... est ------", where "...." is a noun and "------" is an adjective, designates the state of affairs that the designatum of the noun belongs to the class (has the property) designated by the adjective. Accordingly, the sentence "Le soleil est chaud" designates the state of affairs that the sun is hot; it may immediately be translated into an English sentence, namely "The sun is hot".—On the other hand, the semantics of a language which is based on a gradable relation of designation, may contain indications like these: The noun "sol" designates to the degree 0.7 the sun; the adjective "cal" designates to the degree 0.9 the property of being hot; further, there may be the following rule: A sentence of the form ".... esti ------" where "...." is a noun and "------" an adjective, designates to a certain degree the state of affairs that the designatum of the noun has the property designated by the adjective; and the degree is the product of the degrees to which the noun and the adjective designate their designata. Accordingly, the sentence "sol esti cal" would designate, to the degree 0.63, the state of affairs that the sun is hot. As a comparison with the preceding example shows, this sentence would not admit of a translation into English; in particular the statement we made about its designatum, does not furnish such a translation. Thus we see that if a theoretical linguistic system is built up by means of a gradable relation of designation, the language thus arrived at is of a very strange kind; it is not an interpreted language in the usual sense.

Therefore, there is no place for a gradable concept of designation and hence no place for a purely semantical concept of vagueness either, when a system of syntax and semantics is to be abstracted from a natural language, or when an artificial language is to be constructed which is to serve as an interpreted linguistic system for communicating empirical contents. Thus the question as to the influence of vagueness upon the validity of the principles of logic does not arise on the purely syntactical and semantical level of investigation, and no modification of the logical symbolism is necessary.

8. There is, however, another respect in which the vagueness of the terms of a given language may suggest a modification of its logical structure. As has been pointed out in section 6, vagueness is, so to speak, an ineradicable feature of any interpreted language; it can never be completely suppressed. But vagueness (in the strictly semiotic sense explained in section 1) may assume higher or lower degrees, and there are certain ways of modifying a given language in such a way that its vagueness in practical use is decreased.

As vagueness obviously is a serious obstacle in establishing hypotheses and theories which are intersubjective, i.e. which may be tested, with the same result, by different observers, it is particularly important to diminish as far as possible the vagueness of scientific terms. The method which is applied for this purpose, consists in that a vague term which has so far been used as an undefined concept or has been explained by means of other comparatively vague terms, is defined (or redefined) in terms of less vague concepts.

There is one form of this procedure which deserves special mention here. It consists in reintroducing such concepts which have originally been used as names of properties, in such a way that they become names of relations. Take, for example, the concept "hard" as applied to physical materials. In everyday language, this term is used as the name of a property which does not have a precise definition. It will be said to apply to granite and not to cold cream at room temperature; but there will be many ≫doubtful cases≪; "hard" is a rather vague term. For its use in mineralogy, it has been replaced by a term which is introduced by the following definition: If, with an edge of one mineral, it is possible to scratch a second mineral, and if the inverse does not hold, the first mineral is called harder than the second.—The term "hard" has thus been replaced by the term "harder than" which does not designate a property of a substance, but a relation which may obtain between two substances. One of the advantages of this procedure is that the vagueness of the second term is considerably smaller than that of the first, because "harder than" has been defined by means of a criterion which admits of an intersubjective application and which is formulated

in terms which are less vague than the term "hard" of everyday language.

The above definition obviously renders the concept of hardness gradable, without, however, defining numerical degrees of hardness; in fact, it determines what may be called a purely topological order of all substances according to their hardness. In most cases, the development goes a step further; it transforms the vague property-concepts of everyday language, such as "long", "heavy", "hot", "expensive", into metrical terms which designate numerically gradable magnitudes, such as "length", "weight", "temperature", "price".[12] Here again, the defined metrical terms are less vague than the mostly undefined corresponding concepts in everyday language.

As has already been mentioned, the more precise concepts which are thus introduced have a different logical structure than the corresponding vague terms of customary language. The latter are, in our examples, property-concepts (one-place predicates); their scientific successors are either relation-concepts (two-place predicates), like "harder than", or metrical terms (numerical functors), like the term "temperature" which is used in sentences like "The temperature at this place is now 19°C".— This is the sense in which vagueness may really influence logic: it may suggest (though not require) a modification of the logical structure of scientific concepts.

The transition from "L (x)" to "L (x, C)" which Mr. Black suggests, seems to me to belong into this very context. It is a proposal to replace any vague one-place predicate "L" by a metrical expression; a transition which corresponds to that from "x is hot" to "the temperature of x in centigrades is t". But this transition does not involve any modification of the principles of logic; thus, for example, it remains true that either the temperature of x is t, or the temperature of x is not t; and analogously, Mr. Black certainly wants to maintain the principle of excluded middle with respect to expressions such as "L (x, C)".

By his definition of "L (x, C)", Mr. Black makes the interesting at-

[12] For more details about this development in the formation of scientific concepts and about its logical foundations, cf. Carnap {3}; Hempel and Oppenheim {1}; Oppenheim {1}.

tempt of laying down a general rule which allows us to metricize any vague predicate "L"; in fact, the numerical degree C is defined, for every object x, by means of the number of those decisions in which observers apply "L" to that object, divided by the number of the (analogously defined) decisions to the contrary. This is, however, not the way in which gradable magnitudes are introduced in science; thus, for instance, the temperature of a body is not defined by reference to the percentage of observers who apply the term "hot" to it, but by means of more objective criteria such as the dilatation of a mercury thread or of another test-substance; the advantages of this method for the establishment of intersubjectively testable and unambiguous statements are obvious.

Certainly, there is no logical objection to introducing gradable concepts in the way which Mr. Black proposes; it amounts to determining the gradations of a certain property by reference to the judgment of a sufficiently great number of experts. Methods of this kind are in fact sometimes made use of; but they form only the starting point for a definition by more objective criteria.[13]

In fact, gradable concepts which are introduced by objective criteria prove more fruitful for the establishment of empirical laws (such, as, say, the law connecting the volume, the temperature, and the pressure of a gas). Furthermore, concepts which are introduced by the method proposed by Mr. Black, involve ambiguities. If, for instance, we are told that a certain surface possesses the colour yellow to the degree 4 in Mr. Black's sense, then we know only that $\frac{4}{5}$ of all observation judgments passed on the colour of the surface, are in favour of applying "yellow", whereas $\frac{1}{5}$ of them are against it. But this result covers several very different possibilities as to the colour of that surface; in fact, the latter may differ from a pure yellow by a reddish or by a greenish shade, and in other ways, all of which can be separated conceptually by those methods of graduating colours which are used in science.

9. The main points of this paper may be summarized thus: Vagueness is a gradable relation of strictly semiotic character; it involves, besides the vague term and its subject matter, also the users of the language in question. Vagueness is ineradicably connected with all terms, logical as well as descriptive, of any

[13] Cf. Hempel and Oppenheim, [1].

interpreted language.—In a language as a system of linguistic utterances of certain persons, each principle of logic will sometimes be violated. The vagueness of all linguistic terms is one, though not the only source of such violations.—Vagueness is strictly semiotic: there is no analogue to it on the purely syntactico-semantical level, on which the logical principles of a language are established. Therefore, the question as to the compatibility of vagueness and logic does not arise on this level. —However, the occurrence of symbols with a high degree of vagueness may suggest a modification in the logical structure of the conceptual apparatus of science, namely the transition from non-gradable to gradable concepts; this procedure is in fact frequently carried out, and it contributes very essentially to a diminution of vagueness in scientific language.

New York.

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Border-Line Cases, Vagueness, and Ambiguity

BY

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THIS paper is concerned with two closely related problems: the first is the general question of border-line cases; the second is a suggested identification of the notions of ambiguity and vagueness. In the first part of the paper I propose to discuss border-line cases in the following way: I shall say what is meant by "border-line cases," discuss their genesis, enumerate and evaluate the different methods of resolving such cases, and make a brief comment or two the bearing, if any, that such cases have on logical theory. In the second part it will be suggested that the notion of vagueness can be reduced to the status of a special but important case of ambiguity; I hold that such a reduction would effect both an economy and a clarification.

In the consideration of these problems, I shall make use in part of the terminology found in the monograph "Foundations of the Theory of Signs" by Professor C. W. Morris.[1] The specific terminology that will be used is the following. A sign may be said to denote any object which has certain properties . . . as the sign "chair" denotes anything with a raised seat and a back

[1] *Foundations of the Theory of Signs:* Charles W. Morris. *International Encyclopedia of Unified Science*, Vol. I, no. 2; The University of Chicago Press, 1938. I am greatly indebted to Professor Morris for a number of points expressed in this paper. I have also to thank my friend Dr. Bruce Waters for valuable criticism and help in preparing this paper.

et cetera. The class of such objects is said to be the "designatum" of the sign; and the sign will be said to "designate" its designatum. The objects which are denoted by a given sign will be called the "denotata" of that sign. In general, with every sign there is associated a designatum, which is a class. This class may be null, in which case there will be no objects denoted by the sign. When the class is not null, its members are denotata of the sign in question.

Professor Morris distinguishes three dimensions in the sign-process, where the sign-process is to be described very roughly as the taking account of something by somebody through the mediation of a sign. The "syntactical" dimension concerns the relations which signs have among themselves; the "semantical" dimension concerns the relations holding between signs and their designata and/or denotata; and the "pragmatical" dimension is concerned with the relations holding between the sign and the users of the sign, i.e., the interpreters of the sign. I shall make use of another term in semiotic which did not appear in the monograph, the "significatum." If the designatum be regarded as equivalent to the class of denotata, then the significatum may be regarded as the class-defining characteristic or property, and may be said to be "signified" by the sign.

I. BORDER-LINE CASES

Let us first distinguish between three aspects of signs that are generally held to be distinct: generality, vagueness, and ambiguity. A term is said to be general if it is possible for there to be a multiplicity of objects denoted by it, while it preserves the same 'meaning', that is, if it has a multiplicity of denotata when understood in a single sense. A term is said to be ambiguous if it is really two terms which are identical in phonetic and written form, or if it has a number of different 'senses' or 'meanings' connected with it. Put more precisely, a term is said to be ambiguous if there exist a plurality of designata associated with it, or designated by it. A term is said to be vague if there exist objects such that it is not possible to decide, even when given all the relevant evidence, whether the objects are denotata of

the term or not. (The phrase "not possible to decide" is perhaps not the best term to use, but the meaning will probably become clearer as the paper proceeds.)

The word "chair" serves as an illustration of all three of these properties. The word "chair" is general, in that it applies to a number of objects: there are many things in the world which we denote by the word "chair". The word "chair" is ambiguous, since we can use it in different 'senses': one can sit down on a chair, or one can hold a chair at a university. Finally, the word 'chair' is vague, because there exist objects such that some people who know perfectly well how to use English will disagree as to whether the objects in question are "properly to be called chairs". That is to say, the word "chair" is vague because there are border-line cases associated with its usage.

It is usually recognized that the generality of a term (or of some terms) is an advantage and not a defect of language; but ambiguity and vagueness are held in lower esteem. There is no question but that ambiguity is of very doubtful value in scientific discourse; but it may be of great value in other types of discourse, such as literary. For example, it is of value in poetry, where it is part of the poet's purpose to arouse a multiplicity of images and so to obtain a complex response. Further, puns depend upon the ambiguity of the terms which are 'punned on'. It is to be admitted, however, that neither vagueness nor ambiguity have a place in scientific discourse. It is held by some that an ambiguity can, in principle at least, always be resolved. We shall consider at somewhat greater length the phenomenon of vagueness.

The vagueness of a word has been defined in terms of objects such that it is not decidable whether the word applies to the object or not. When we come upon such an object or situation in using a word, we shall call it a 'border-line case'. Such cases have been described in various ways: Max Black[2] writes: "... the set of all objects about which a decision as to the symbol's application is intrinsically impossible is defined as the

[2] *Vagueness:* Max Black. Philosophy of Science, Vol. IV, no. 4, pp. 427 ff.

'fringe' of the symbol's field of application." Morris R. Cohen[3] calls such sets of cases "twilight" or "penumbral" zones. When a person encounters a situation concerning which it is impossible to decide whether to apply or to withhold a particular term, he may be tempted to toss a coin to decide. We may characterize a border-line case as any case in which one is tempted to decide by tossing a coin.

There are three types of signs which we shall not consider. These are first; nonsense syllables, which have not been defined and which we should not think of applying to any objects: second; contradictory terms, such as 'round-square': and third; universal terms, such as 'thing', 'entity', 'being', etc. We need only exclude the first, of course, since the second type, having null range, and the third, having universal range, are hardly susceptible of having border-line cases.

It is a fact of experience that we learn to use some words independently of explicit (i.e. verbal) definition; learning instead by having some of their denotata pointed out to us. Some words, that is to say, are defined only ostensively. Over and above its being a fact of experience, it is necessary that there be *some* words whose meanings are known without explicit definition if we are to have any contact through language with things that are non-linguistic. This is so because explicit definitions are made in terms of other words; hence if we demanded explicit definitions for all words, we should be involved either in circularity or a regression, of vicious sorts. It is further clear that in learning to use words in this way we did not have *all* the denotata indicated to us in all cases. Put in class terminology, we are given the class name, and have presented to us a part of the extension of the class, but we are not given the class defining characteristic. In the language of semiotic, we are given the sign and some, not all, of the sign's denotata, but are not given explicitly the designatum or the significatum of the sign.

From this point on in the paper we are to be concerned only

[3] *Concepts and Twilight Zones:* M. R. Cohen. Journal of Philosophy, Vol. 24, no. 25, pp. 673 ff.

with words which we know how to use correctly *in most cases*. But we shall be dealing with vague words, such that we do *not* know how to use them correctly *in all cases*.

Let us turn our attention now to terms whose usage is familiar, but which are without explicit definition. When we encounter a border-line case in using such a term, we may not bother to take the steps (whatever they may be) necessary for reaching a decision as to the term's application. For instance, we do not always make, or even try to make a decision as to whether a purported work of art is 'good' or 'beautiful'. But if for some reason or other we feel impelled to decide, we may make our decision in any of the following ways:

1) We can toss a coin to decide whether to apply the word or its contradictory. (e.g., 'good' or 'not-good'.)
2) We can deny that we have to choose between the word or its contradictory, that is, we can deny the principle of the excluded middle. This procedure would involve either giving up logic altogether, or constructing a three or an n-valued logic, in the belief that this will be more applicable to our language. This case will be discussed in a later section of Part I, as a purported bearing of border-line cases on logical theory. For the present, we shall not consider it.
3) We can attempt to solve the problem by devising a definition for the term, such that the application of the definition will enable us to reach a decision concerning the case in point. In constructing such a definition, we usually try to make it consonant with former usage of the term, as ostensively determined. This is not always necessary, however, and our definition may prove to be more than a mere extension of former usage.

Let us try to see exactly how such a definition as suggested in (3) is to be constructed, and to isolate the factors which are involved in such a process. (We shall, as has been said, postpone any discussion of (2), and while it is clear that (1) may sometimes be invoked, it is in general not significant.) We are dealing here with terms exhibiting generality, and further with terms that have particular objects as their denotata. That is to say, we

are confining ourselves to the consideration of words occurring in that part of the language of ordinary discourse in which we talk about such things as tables and chairs. For a term to be general, as has been said, it must be potentially applicable to more than one object. Now for two objects to be denotata of one word, they need be similar only with respect to certain relevant properties (or one relevant property). These relevant properties are the significatum of the word. For instance, we call two objects both chairs in spite of differences of color, of position, of size (within limits), etc. We may call two extremely dissimilar objects by the same name, if only they have the relevant property or properties in common. On the other hand, when two objects are sufficiently dissimilar with respect to the relevant properties signified by a sign, we do not denote both objects by that sign. It is clear that we may know this independently of any explicitly formulated definition of the sign in question; since to use a term correctly we must make the distinctions here outlined, and we do use some terms correctly without having explicit definitions for them.

In the preceding paragraph the word 'relevant' appears several times. This term concerns the pragmatical dimension, since the selection of which properties of the objects denoted by the word in question are to be selected as the 'relevant' properties or the significatum depends upon the users of the sign, or the interpreters. *The rule of usage for a term which has not been explicitly defined is an unstated semantical rule, but the necessity for the selection of relevant properties makes our rule of semantics dependent somehow upon the pragmatical factors which actually determine our selection.*

The occurrence of border-line cases impels us (sometimes) to formulate explicitly a semantical rule for the sign in question. We must make a decision as to which properties of the objects hitherto included as denotata we are to consider relevant. This is a complicated and difficult task, since it is highly improbable that we should ever be able to enumerate completely all the common properties of any two objects, however dissimilar.[4]

[4] That is, for objects such as tables and chairs. I should not care to make the statement for entities of a higher logical type, or for entities on different type levels.

It is certainly the case, however, that as we ordinarily use language, two objects must possess some properties in common before we apply the same term to them, or consider them both to be denotata of the same sign. For the type of word we have been considering up to now, however, these properties have not been explicitly stated. One of the goals of an 'analysis' of a term is an explicit statement of the properties which must be shared by the denotata of the term. We define our word, therefore, in terms of several other words: where the definiendum is the word whose vagueness we wish to diminish, and the definiens consists of words denoting the relevant aspects of the objects to be denoted by the definiendum.

There are degrees of vagueness, that is, one term may be less vague than another. For example, the word 'scarlet' is less vague than the word 'beautiful'. We may say that one word is less vague than another if the first is less susceptible of border-line cases than the second. It is desirable that the definiens be less vague than the definiendum, so that the definition will give us a method whereby to resolve some of the border-line cases connected with the definiendum. (It is to be observed that there are other types of definition, namely those of 'manufactured' terms, introduced for purposes of convenience of notation, which are completely undetermined before they are defined to be equivalent to a complex definiens. Definitions of this type cannot be incorrect or false; whereas we may strongly feel that a definition which is the result of an analysis is 'false', or 'incorrect'. When this occurs we say that the analysis was faulty which resulted in the 'incorrect' definition.) In the definition resulting from an analysis, the terms composing the definiens are vague themselves, and consequently the definiens is itself vague. From this it is apparent that we have not eliminated the vagueness of the definiendum; we have at best merely diminished it. As we proceed, by repeated analysis culminating in repeated definitions, to delimit more and more narrowly and precisely the scope of the original definiendum, we may hope to do it only in terms of words that are themselves less and less vague. We cannot assume an attainable limit to this process, even though we tend towards absolute precision as a limit. This

last may be a consummation devoutly to be wished; but it is perhaps not even theoretically possible; and it is very questionable whether it would not be incompatible with aspects of language other than the strictly scientific.

As we have seen, any border-line case may be resolved by means of an analysis and definition of the term; but this is by no means equivalent to saying that all border-line cases of a term may be resolved by repeated analyses and definitions. This is so because there are ultimately some words in our language (or in any empirical language) which cannot be so analyzed and defined. Any particular word may be so treated, but it is not the case that all words are explicitly definable. This is so because of the circularity or regression involved. These remarks are not intended to deny the value or the efficacy of such analysis; but only to point out the limitations of the method.

Let us now turn our attention to terms which have been analysed and for which explicit definitions have been devised. There are several ways for a border-line case to arise in the case of terms which have been so defined. But first let us state what sort of definition or semantical rule is usually obtained: the general schema is

O is to be denoted by "W" if and only if $w_1(O), \ldots, w_n(O)$.[5] where O is the object in question, or any object; "W" is the word we are defining, or *talking about*; and "$w_1, \ldots, w_n$ are the words we are *using* to define "W". For example: O is to be denoted by the word "chair" if and only if O has a raised seat, O has a back, etc. Now for a word which has been analysed, and which has an explicitly formulated rule or definition (in addition, that is, to its ostensive one), border-line cases may arise in any of the following ways.

i. Where it is not known whether or not O possesses all the 'minimum properties' listed, or signified by the definiens.
ii. Where it cannot be determined (i.e. decided) whether or not all of the w's apply to O.
iii. Where it is known that the definiens denotes O, but O has

[5] Stout has suggested "any m of n listed properties". The comments made in this paper would remain essentially unchanged if his formulation were adopted.

some other property or properties which incline the user of the definiendum to withhold it from O.

iv. Where it is known that some one or more of the w's of the definiens does not apply to O, but O has some other property or properties which incline the user of the definiendum to apply it to O.

v. Where it is known that the definiens denotes O, but O has some other property or properties which disincline the user of the definiendum to use the definition as arbiter in deciding whether to apply the definiendum to O or not.

vi. Where it is known that some one or more of the w's of the definiens does not apply to O, but O has some other property or properties which disincline the user of the definiendum to use the definition as arbiter in deciding whether to apply the definiendum to O or not.

The cases listed are all distinct, although there are several other ways for border-line cases to arise. For example, i and ii could stand the sort of 'unpacking' that v and vi are of iii and iv. But these are enough to illustrate the general sorts of difficulties involved.

In cases i and ii, the interpreter is willing to apply the definition, but is not able to do so. In cases iii and iv, the interpreter is able to apply or to invoke the definition, but is inclined to disregard the definition, on the grounds that it is 'wrong' or 'mistaken'. Cases iii and iv are perhaps not strictly border-line cases, since it is believed that the definiendum applies, or that it does not apply, but does so contrary to the earlier analysis and definition. But the situations described in iii and iv are difficult cases, and arise in connection with the results of earlier border-line cases. Cases v and vi are more properly border-line cases, as here the interpreter is able to apply the definition, but not willing. Cases i and ii could be further unpacked into cases where the interpreter was not only unable to apply the definition, but also unwilling to do so.

In cases i and ii, the vagueness is pushed back into the definiens. In cases iii and iv, there is no vagueness, strictly speaking, but a confusion; the definitions are discovered to be 'false'.

In cases v and vi, the definiens is not vague in context, i.e. the case is not borderline as far as the definiens is concerned, but the case is borderline for the definiendum in spite of the definition. In the sort of cases that i and ii could be unpacked into, where the interpreter is not only unable to apply the definition, but unwilling, the case in question would be doubly-borderline, in that it is a borderline case for the definiens, and in addition is the sort of borderline case that we find occurring in v and vi.

To resolve cases iii and iv, we must revise the definition. We must also revise our definitions in the other cases; but a consideration of the last sort of case mentioned in the preceding paragraph shows us that with further definitions we may come upon triply and n-tuply borderline cases. But in general the process of analysis and definition will diminish the susceptibility of the definiendum to borderline cases.

The repetition of the process of analysis and definition will not solve all of our difficulties with border-line cases; but it will solve any difficulty that arises.

Before going on to Part II, let us consider briefly the purported bearing of these border-line cases upon logical theory. This clearly is concerned with the gap between theory and practice: as between the impossibility in practice of arriving at a decision with respect to a border-line case; and on the side of theory, with the Law of the Excluded Middle. The situation is perhaps best described by means of an analogy.

Suppose that we attempt to apply simple rational dynamics to a very crude machine, whose wheels are only approximately circular, and whose 'straight, rigid bars' are wavy and flexible. The attempted application will simply not yield desirable results. There are three attitudes that we might assume with respect to this situation. We may improve the machine, making its wheels more nearly circular, and straightening and strengthening its rods; or we may complicate our mathematics, so as to take account of the deviations from 'normal' of the parts of the machine; or we may simply declare that the mathematics involved is 'false'. There are three strictly similar attitudes open in considering the case of the application of formal logic to vague terms.

I. The first attitude has been assumed in this paper thus far. The machine to which our theory is being applied is improved so as to make the theory more precisely applicable. The vague terms are repeatedly analysed and defined so that they will more nearly conform to the 'laws of logic'.

II. The second attitude was exemplified by Max Black, in the interesting paper referred to earlier.[2] In that paper the problem is faced squarely, and a very ingenious method is given for the treatment of border-line cases. In his paper the 'objective existence' of border-line cases is established, ('objective' being defined by Black to mean independent of this or that particular observer); and the precise application of formal logic to vague terms is effected by the introduction of a new variable into the argument, the new variable being roughly an empirically determined 'vagueness coefficient' (Black does not call it by this name) for the vague term in question. As the terms approach absolute precision as a limit, this extra variable tends to drop out, or to go to zero; and so his symbolism is an extension of ordinary symbolic logic, which is possessed of all the validity of the former in the limiting case, and which is applicable with precision to a wider field than is the original. Mr. Black's device would seem to have met successfully the problem of the application of symbolic logic to terms which are not absolutely precise, or at least to have pointed the way, for his explication of the methods of formulating the extra coefficient is not complete for all cases, and much important detail remains to be treated.

III. The third attitude is the second one listed in discussing what to do about border-line cases in general: where 'ordinary' logic is thrown out, and an attempt is made to invoke a three or an n-valued logic. As I understand it, there are reasons for the construction and investigation of three and n-valued logics, but the vagueness of terms and consequent existence of border-line cases cannot be counted among these reasons. That is so for the following reason. The vagueness of a term militates against applying to the term the law of the excluded middle. For a border-line case the interpreter does not know whether to apply or to withhold the term (here 'does not know' does not mean 'has not sufficient evidence', for the essential thing to get straight

in considering border-line cases is that they are undecidable even when all the evidence is at hand.) In such a case the law of the excluded middle may be said to fail, inasmuch as we do not say either that the term applies, or that it does not.[6] In the case of the vague term P(x) it is impossible to assert the existence of a definite line drawn between the cases x such that P(x), and the cases y such that $-P(y)$. One may be tempted to assert that there are three truth possibilities, and to invent a law of the excluded fourth, dividing all things to which the term P is potentially applicable (that is, which are of the proper logical type) into three divisions: those cases x such that P(x), those cases y such that $-P(y)$, and those cases z such that 'maybe P(z)'. But there may arise border-line cases such that one does not know whether to apply P or 'maybe P'. At any rate, it is quite obvious that where one cannot draw one line, so as to exclude a third, *a fortiori* one cannot draw two lines so as to exclude a fourth. Perhaps no one has actually made the mistake described, but Korzybski[7] writes as though he had; and a few others. At any rate, this procedure is clearly not a solution to the problem at all.

II. VAGUENESS AND AMBIGUITY

I intend in Part II to present various arguments designed to support the view that vagueness is a special case of ambiguity, rather than a distinct and independent property of language. In addition to the factor of economy, which is not to be minimized, such a reduction is equivalent to a clarification and an explanation of the causes of the vagueness of terms.

It will be simpler to begin with an examination of the border-line cases arising in connection with those terms which have both an ostensive definition and an explicit one. It is clear that for at least the last four of the six types of border-line cases listed in Part I, the following situation obtains. The term in ques-

[6] The Law of the Excluded Middle may be formulated in either of two ways: ontologically, as "A is either B or not-B"; or semantically, as "For any A, either B can be predicated truly of A, or not-B can be predicated truly of A." It is of course only with respect to the second formulation that a border-line case may be said to occasion difficulty.

[7] *Science and Sanity:* Alfred Korzybski. Lancaster, Pa., 1933.

tion has been ostensively defined; and this ostensive definition has given us one (implicit) semantical rule for the use of the term. The term in question has been also verbally defined, and this verbal definition has given us a second (explicit) rule for the use of the term. In the border-line cases arising in the way indicated in ℵiv[8] on page 189, we have a situation which is a denotatum of the term in virtue of the first semantical rule, but which is not a denotatum of the term in virtue of the second semantical rule.

This is tantamount to saying that the sign in question has two designata associated with it; and these two designata are different, inasmuch as one has the border-line case in question as a member, being denoted by the sign, and the other does not include the border-line case as a denotatum. But this plurality of different designata is precisely our definition of ambiguity, as stated in Part I. This is the sort of analysis that seems to indicate that vagueness is a sort of ambiguity, although admittedly a complicated sort.

Vagueness is a complicated sort of ambiguity in that it is quite different from what is usually connoted by the word 'ambiguity'. What one usually thinks of in connection with an ambiguous term, is an equivocation that can easily be resolved. That is, an ambiguous word is usually thought to have a number of distinct and easily distinguished 'meanings' or designata. The kind of ambiguity that belongs to a vague term is *not* easily resolved, because the different designata associated with it overlap for the most part. This overlapping is what is meant by saying that the explicit definition is to be 'consonant with former usage of the term, as ostensively determined' (page 185). To make this point clear, I shall say a word about puns, and then adduce an example of a border-line case which is clearly the result of this sort of ambiguity.

When a situation is punned on, what is being done very often (and I believe this sort of procedure to be at the basis of all punning) is the using of one sign-vehicle, which designates two

[8] We focus our attention on the particular case iv only for the sake of greater definiteness. The analysis holds for any such type of case, with only the most minor alterations.

designata, or is governed by two semantical rules, to denote one single situation *in virtue of both semantical rules*. The satisfaction connected with the pun is the surprise and realisation that the denoting is two-fold, i.e., that two ordinarily mutually exclusive designata both include the denoted situation as their common part.[9]

An illustrative example of a border-line case which is the result of the ambiguity of a vague term is a cartoon showing Mutt and Jeff in a Zoo, standing before the enclosure containing the giraffe. Mutt is pointing at the giraffe and exclaiming "There ain't no such animal!" I believe the following to be a plausible explanation of the situation cartooned. The giraffe constituted a borderline case (which Mutt was in the process of resolving by main strength) with respect to the vague term 'animal'. The vagueness of the term can be resolved, I think, into the sort of conflict of semantical rules which has been discussed more generally above. Mutt had an ostensively given semantical rule for the use of the word 'animal', which he had learned by having specimen animals indicated to him. This habit of usage would exclude the giraffe from the class of animals. On the other hand, according to the rule that anything is an animal if it is contained as inmate by a Zoo, the giraffe *is* an animal. Thus the vagueness, in this case at least, is resoluble into a conflict of semantical rules; and this plurality of semantical rules is what we have agreed to call ambiguity.

The vagueness of a term which has been defined only ostensively presents a more difficult problem. Instead of discussing this case in general, let us take a particular word, and direct our investigation specifically at it. It is to be hoped that the discussion can be generalized to account for any such word. Let us choose the word 'red' as a suitable example. Now there are shades such that they cannot be said to be red, or not to be red. The existence of such border-line cases manifests the vagueness of the term 'red'. This case is as difficult as may be imagined, because of the fact that 'red' is defined *only* ostensively. Discovering the ambiguity of a word that is susceptible of only one

[9] At least, there is the two-fold denoting mentioned. Whether or not this is the source of satisfaction is a matter for the aesthetician to decide.

method of definition is at first glance a difficult task. But I believe it to be possible. Before proceeding, I should like to admit that this part of the argument seems somewhat less convincing and more tentative than the foregoing, and that one of the reasons why I put as much stock in the following considerations as I do, is that if they are valid, the treatment of vagueness is made uniform; whereas if they are not, there is a troublesome bifurcation between the treatments of two kinds of vagueness.

With a term like 'red' there are differences of kind: for instance, there are light reds, and medium reds, and dark reds. It is further the case that light red is more similar to medium red than it is to dark red. (I have no desire to become entangled in the realist-nominalist-conceptualist morass; and so I hope that the view here presented is compatible with any of these three theories. I think that similarity and degrees of similarity are admitted on any of these three views; and similarity is all that is being invoked here.) The ostensive definition of a term seems to depend upon similarity: if something else is to be called by the same name as this sample thing which *is* called by the name, it must be similar to this thing. Let us consider a border-line case, with reference to the term 'red', where the color in question is 'between' red and orange. Here I think the following situation may be said to obtain. The case in question is sufficiently similar to very light red things known to be red to impel us to call it red itself. On the other hand, it is sufficiently dissimilar to certain medium and dark red things known to be red to impel us to deny that the questionable case is red. Here two conflicting semantical rules seem to be operative with reference to the one word 'red'. And this is what we have agreed to call 'ambiguity'.

It is to be admitted that the above account is too psychological to be entirely satisfactory; but it has the merit of making plausible the desirable view that all vague terms, or cases of vagueness, can be treated in the same way.

It may be urged that the last few paragraphs depend upon the vagueness of the word 'similar', and that this is unanalyzable. This may be a genuine obstacle, and I do not see how to get round it myself; but I am of the opinion that it is not insuperable.

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Concerning the Laws of Contradiction and Excluded Middle

I

BY

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TRADITION usually assigns greater importance to the so-called laws of thought than to other logical principles. Since these laws could apparently not be deduced from the other principles without circularity and all deductions appeared to make use of them, their priority was considered well established. Generally, it was held that the laws of thought have no proof and need none, that as universal constitutive or transcendental principles they are self-evident.

There have been many dissenting opinions, of course, and many impressive systems erected upon a deliberate violation of these laws. Hegel, for example, held all three in contempt. The law of identity is empty and meaningless, apparently more empty and meaningless than any other proposition. The law of contradiction is contradictory, while the law of excluded middle, as usually interpreted, is "so insignificant that it is not worth stating." On the other hand, one of the most basic doctrines of the Hegelian philosophy was that reality cannot be contradictory, which is simply one interpretation of the law of contradiction. In a certain sense Hegel took the law of contradiction more seriously than those contemporary philosophers, who, while decrying his dialectic as madness, reduce the principle to an arbitrary verbal convention. The usual objections to the laws of thought that

they are abstract and meaningless, that they are static and inconsistent with change, that they are psychological limitations or verbal conventions do not represent the majority opinion, which has held them to be prior to, and hence, more important than other logical principles.

The traditional attitude toward these so-called laws of thought was bound to change with the development of the postulational method, just as the geometers' outlook was changed by the appearance of non-Euclidian geometries. Mathematicians entering the field without the accustomed reverence for logical precedents, constructed postulate sets in which the three laws did not occur. Not only were they unnecessary as premises, but it appeared in Whitehead and Russell's *Principia Mathematica* that they could be deduced from other logical principles, and could therefore lay no claim to priority. This opinion which is widely held today is a complete reversal of the traditional view. The laws of thought are now generally assigned the same status as other logical principles. One prominent school of thought, for example, reduces them all to tautologies. As Wittgenstein put it: "All these propositions say the same thing. That is, nothing."

Although this is the dominant view at the present time, it seems in view of the following considerations, very doubtful. The fact that the laws of thought do not appear among the formal premises of a system does not mean that they are not involved in the proofs.

"This problem," Johnson says, "is perhaps of purely technical interest, and the attempt at its solution presents a fundamental, if not insuperable, difficulty: namely, that the procedure of deriving new formulae from those which have been put forward as to be accepted without demonstration, is governed implicitly by just those fundamental logical principles which it is our aim to formulate explicitly. We can, therefore, have no assurance that, in explicitly deriving formulae from an enumerated set of first principles we are not surreptitiously using the very same formulae that we propose to derive. If this objection cannot be removed, then the supposition that the whole logical system is based upon a few enumerable first principles falls to the ground." (Vol. I, p. 223.)

The importance of *Principia Mathematica* owed much to the fact that it put down on paper more of the premises required for its proofs than any comparable system had ever done (with the exception of Frege's logic), but as Johnson suggests, there were still many loopholes for intuitive steps and concealed premises. In logic, as in no other science, the rules of inference employed in proving theorems are among the theorems to be proved. Since rules of inference such as: "What follows from a true proposition is true" are not put down as premises in the arguments, and cannot be, as Lewis Carroll demonstrated, there is always a possibility that in proving a proposition we are using the same principle, or one logically dependent upon it, as a rule of inference.

Thus the rule of inference, "What follows from a true proposition is true", is used to prove the theorem, *3.35, which is the same thing in symbolic form. In general, the rule of inference insures the assertion of non-self-contradictory propositions. If what followed from a true proposition were ever a false proposition, then self-contradictory propositions would occur in the system. The rules of substitution, which are not explicitly stated in *Principia Mathematica*, also open up the possibility of this kind of circularity in the proofs. For example, if a proposition p occurs more than once in a proposition we are proving, we are allowed to substitute $\sim p$ for every occurrence of p. But we are not permitted to substitute $\sim p$ in one place unless we also substitute $\sim p$ in every other place in which p occurs. Thus, in $p \equiv p$ (p is materially equivalent to p) we can substitute $-p$ for p in both places but not in one place only, for in that case we should have $p \equiv -p$, which contradicts the definition of equivalence. It is clear that the rules of substitution serve to exclude violations of the law of contradiction and the law of excluded middle as well. The rule of inference, likewise, not only enables us to drop our premises but to exclude self-contradictory propositions.

Definitions in *Principia Mathematica* also merit close attention since, as the authors themselves point out, they often convey more important information than is contained in the theorems.

"A definition," Russell says, "is concerned wholly with the symbols, and not with what they symbolize. Moreover, it is not true or false, being the expression of a volition, not a proposition. (For this reason definitions are not preceded by the assertion sign.) Theoretically, it is unnecessary ever to give a definition: we might always use the definiens instead, and thus wholly dispense with the definiendum." (*Principia Mathematica*, vol. 1, First Edition, p. 12.) He also remarks that he does not need to define definition or to introduce it as a primitive idea because definitions are "no part of our subject, but are, strictly speaking, mere typographical conveniences." Three comments can be made at this point. First, since definitions convey information and give clarity and definiteness to expressions, as Russell claims, they would seem to make an assertion; and thus to be true or false. Russell seems to conclude that because they do not make assertion about the subject-matter, i.e., elementary propositions, they do not make assertions at all. It is more reasonable to suppose with G. E. Moore that what they assert is that in *Principia Mathematica* a certain expression, the definiendum, will be used as short for another, more analytical expression, the definiens. In this sense the definition is true if the authors are consistent. Russell's assertion that definitions are mere typographical conveniences and that the avoidance of cumbersome expressions and complications is their only service is also, of course, very doubtful, in view of the fact that the transition to all new topics is effected by definitions. Instead of starting off on a new subject with new axioms, as Peano does in his articles in the *Formulaire de mathématiques*, Russell begins with definitions. This procedure, while very useful, leaves open the possibility that new axioms are being introduced in the guise of definitions, and that *Principia Mathematica* contains more primitive propositions than the authors believe. A definition is merely verbal, Russell says, and no part of the subject, and yet, by means of the theorem, $\vdash \cdot p \supset p$, any definition can be transformed into an equivalence, into an asserted proposition of the system, into a proposition not about words, but about things. Definitions are often of crucial importance. Without the defi-

nition of conjunction, $p \cdot q = \sim(\sim p \vee \sim q)$Df., which is one form of DeMorgan's principle, Whitehead and Russell's deduction of the law of contradiction from the law of excluded middle could not have been accomplished, nor could their system have retained the same degree of duality and completeness. It will be seen in what follows that the definition of conjunction, like the definition of material implication, leads to important paradoxes, which is another indication that the definitions of *Principia Mathematica* are not mere typographical conveniences unrelated to the subject, and that they are not as innocent as Russell claims.

These considerations are not new, but they are seldom, I think, applied to the point at issue. Taken together they are sufficient to disprove the notion that the laws of thought can without circularity be deduced from other principles. At the same time, they argue for the priority of the laws of thought. An examination of the truth-table method of proving propositions of *Principia Mathematica* reinforces this conclusion. Those acquainted with the writings of Post, Wittgenstein and others who have followed this method will understand that the various logical constants such as implication, disjunction, etc. may be defined in terms of the truth possibilities which they allow. Implication can be defined as holding for every pair of propositions, p q, except when p is true and q false, disjunction, when they are not both false, while conjunction only holds when p and q are both true. With the logical constants defined in terms of true and false possibilities, we can test the truth of all of the elementary propositions. When the truth tables are set up, however, two metalogical principles universally govern the possibilities. One prevents the same proposition p from having both a true and a false sign in its several occurrences and the propositions p and $-p$, when they occur in the same proposition, from having the same sign. The other insures that every proposition has a sign, either true or false. The first principle, of course, is the law of contradiction, while the second is the law of excluded middle. Both are necessary for the proving of the elementary propositions of *Principia Mathematica* by the truth-table method. This is

rendered even clearer by the example of the law of contradiction itself. The law is proved in *Principia Mathematica* by the law of excluded middle, De Morgan's principle and "Identity", and many readers may not realize that another unstated principle is involved, namely, the law of contradiction itself. When the truth-table method of proof is used, however, everyone can see. Only when the possibility of p and $-$p having the same truth value is excluded, can the conclusion, $-$(p and $-$p), be demonstrated. Here, in other words, the law of contradiction is used to prove itself. That a kind of circularity is also involved in the *Principia* proof of the law of contradiction was argued above on the basis of general considerations. First, there is the fact that the rule of inference is used to exclude self-contradictory propositions, and to include consistent ones. This principle is involved in the proof of the law of contradiction. Secondly, the proof makes use of another principle. If p occurs more than once in a proposition (which is the case with almost all of the elementary propositions of *Principia*) the same substitutions must be made in each case. The rule employed here is a variant of the law of contradiction. It seems fairly clear that the sense in which the law of contradiction and the law of excluded middle can be deduced in contemporary logic from other logical principles is a very technical one. The rules of inference, substitution, the definitions, and the truth-table proofs all furnish evidence. In the truth-table proofs of the two-value logics, for example, the laws of contradiction and excluded middle are always employed, and few others are needed. If one were to prove that these two principles have no priority or special importance, it would be necessary to set up a two-value logic in which they are not prior, either formally or informally. This, it seems to me, impossible to do. In the truth-tables it would always be necessary to allow p in its various occurrences only one value, or sign, and to rule that p and $\sim$p have different values and that all elements have some value or other. The laws of contradiction and excluded middle would be necessary here and no other principles by themselves would serve the purpose. This seems to imply that these two laws are prior and more important. It is therefore difficult

to understand why Cohen and Nagel say that "Even if other principles of logic could be derived from the traditional three, that would not make these more important, or more certain, than any of the others." (*Logic and Scientific Method*, p. 182.) "Importance" is a relative term. In relation to deductive systems, one would suppose "importance" could only mean priority or deductive power.

II

Closely bound up with the theory that the laws of thought are prior to the other principles is the much disputed question of their dependence or independence *inter se*. Aristotle,[1] and many others put the law of contradiction and the law of excluded middle in a single formula, p or not −p, disjunction being understood in its exclusive sense. So interpreted, this formula states that p and −p are not both true and are not both false, and accordingly, states the law of contradiction as well as the law of excluded middle. Yet it would seem easy enough to distinguish between them, and Aristotle and most of his followers have held not only that the two laws are distinct but that they are independent, that neither implies the other. In modern two-value systems, on the other hand, the one is regularly derived from the other and definitions, axioms or rules of substitution, are adopted which make it impossible to assert the one without implying the other. Thus in the Boole-Schröder logic the law of excluded middle follows directly from the law of contradiction with the help of De Morgan's principle and the same is true in Lewis and Langford's version of the system of material implication (*Symbolic Logic*, p. 136) and in Lewis's system of strict implication, where the same proof is used. In *Principia Mathematica* itself, the procedure is different. Since disjunction, rather than conjunction, is taken as primitive, the order of the laws is reversed and the law of excluded middle is used to prove the law of contradiction. Likewise, since $\sim \sim p$ in a proposition cannot be replaced by p, the proof is also longer, and involves many more assumptions. It involves at least the following: The

[1] *Categoriae* 10, 13a, 37.

definitions of implication and conjunction and the rule of inference, the procedure of substitutions and three primitive propositions (*1.2, *1.3, *1.6). If, however, $\sim \sim p$ can be replaced by p, the one law follows from the other directly by means of De Morgan's principle. Thus in Boolean algebra or Lewis and Langford's version of the system of material implication, the law of excluded middle follows directly from the law of contradiction with the help of De Morgan's principle, the principle of double negation being assumed in the procedure of substitution.

Whether disjunction, conjunction or even Sheffer's stroke function is taken as primitive in a given system, is optional and whether the principle of double negation is used in the substitions or not, is a matter of convenience. The important thing is that, if the usual assumptions are made, the law of contradiction and the law of excluded middle are equivalent. That is, either both are true or both are false. There is, of course, nothing surprising in the fact that two theorems in the systems we are discussing should be equivalent. Any two theorems of the first section of *Principia Mathematica*, for example, are equivalent. What is surprising is that precisely these two theorems, the law of contradiction and the law of excluded middle, should be equivalent, for an important body of opinion from Aristotle on has held that they are independent, and, more particularly, that the law of contradiction can be asserted without asserting the law of excluded middle while the latter can be denied without denying the former. Thus in the *Metaphysics* (Γ, 3), Aristotle describes the law of contradiction as "the most certain of all principles," "a principle which every one must have who understands anything that is" and "which every one must know who knows anything." His attitude toward the law of excluded middle is very different. Although in the *Metaphysics* (Γ, 7) he apparently wants to prove the law of excluded middle from the definitions of true and false, assuming, of course, the law of contradiction, in another place (*De Interpretatione*, Ch. 9, 19a) he argues that the law of excluded middle does not apply to judgments about the future. With respect to what is actual, the law holds good, but where indetermination enters, as in the case of things

in the future, it fails. "It is therefore plain," he says, "that it is not necessary that of an affirmation and denial one should be true and the other false (determinately). For in the case of that which exists potentially, but not actually, the rule which applies to that which exists actually does not hold good."

This argument of Aristotle was revived some years ago by C. D. Broad in *Scientific Thought* and more recently by J. Łukasiewicz.[2] Its logical force does not concern us here.[3] What interests us in this connection is that Aristotle is ready to reject the universality of the law of excluded middle while asserting that the law of contradiction is the most fundamental and indubitable of all principles. For we have seen that if one law is asserted, the other must be asserted, unless of course, De Morgan's principle or the principle of double negation ($--p = p$) is rejected. But Aristotle, it is clear, accepted both these principles. Thus in *Metaphysics*, Ross Edition, (K.1063b) he states that:

"No intermediate between contraries can be predicated of one and the same subject, of which one of the contraries is predicated. If the subject is white we shall be wrong in saying it is neither black nor white, for then it follows that it is and is not white; for the second of the two terms we have put together (black and white) is true of it, and this is the contradictory of white."

What Aristotle says here is that if x is white (w) and x is not (black or white) then x is white and not white. This only follows, however, if x is $-$(b or w) implies x is ($-$b and $-$w). In other words, the argument assumes a variation of De Morgan's principle: $-$(b or w) = $-$b and $-$w. Although De Morgan's principle was not explicitly stated until the nineteenth century, it was utilized long before. When Aristotle and Leibniz state that p or $-$ p (where disjunction is taken in the exclusive sense)

[2] "Philosophische Bemerkungen zu mehrwertigen Systemen des Aussagenkalküls" in Comptes Rendus des Séances de la Société des Sciences et des Lettres Varsovie, Classe III, Vol. xxiii, 1930. Fascicule 1–3, pp. 51–77.

[3] See "On Mr. Broad's Theory of Time" by R. M. Blake, *Mind* (1925) for a discussion of Broad's view, and "Are Some Propositions Neither True Nor False?" by Charles A. Baylis *Philosophy of Science* (April, 1936) for a criticism of Lukasiewicz.

states the law of contradiction as well as the law of excluded middle, he is assuming De Morgan's principle. Likewise, in the course of the long controversy between the upholders of exclusive disjunction and the defenders of non-exclusive disjunction, the latter naturally maintained that p or q is true except when both p and q are false, while the former contended that p or q is true except when p and q are both false or both true.

In general it appears that "neither A nor B" admits of two interpretations, namely: "not (A or B)" and "(not A and not B)". Common language passes from the one to the other without a pause, and accepts De Morgan's principle in this form automatically. The acceptance of the principle of double negation is also natural and inevitable. Both principles are intuitively certain, and also required for a form of duality characteristic of modern logical systems. If either is omitted drastic alterations in logical systems would be necessary. Yet if both are retained, and the usual assumptions are made, the law of contradiction and the law of excluded middle are obviously equivalent, a thing which most logicians, from Aristotle on, have denied at least by implication. If the principle of double negation is taken in its usual form, $--p = p$, the whole matter, as we have seen, is much simplified. In this case, the law of contradiction and the law of excluded middle are equivalent if only De Morgan's principle and double negation are granted.

In either case the denial of the law involves the denial of the other. But many of the people who are prepared to eliminate the law of excluded middle (or to restrict its generality) have no idea of abandoning the law of contradiction or double negation, and have no objection, so far as I know, to the very obvious principle of De Morgan. On the contrary. They cling to the law of contradiction as tenaciously as the rest of us, and no general interest is evoked by the idea of creating a system which would dispense with it. They see advantages in excluding the one law, but not the other.

The enemies of the law of excluded middle hail from different disciplines and employ different arguments. There are those who argue that classifications in the social sciences which violate

the law of excluded middle are often more useful than those which do not (Dewey); there are the mathematicians in the circle of Brouwer, who contend that the best way to avoid the paradoxes of Mengenlehre is to restrict this law; and finally, there are the philosophers, such as Broad, who think the law stands in the way of any adequate explanation of the fact of change. None of them show any tendency to question the law of contradiction. Yet, as we have seen, the two laws stand or fall together if the customary assumptions are made. The dialectians, on the other hand, in so far as they deny both laws, and not merely one, would appear to be in a better position.

It may be, of course, that our intuition that the two laws are logically independent is erroneous, that they are, in fact, (materially) equivalent. When one adopts this alternative one is caught on one horn of the dilemma. If the two laws are equivalent, the violation of one involves the violation of the other. The assertion that a certain man is neither bald nor non-bald implies that he is both. Aristotle's plausible (if false) contention that judgments about the future are neither true or false entails the dubious proposition that judgments about the future are *both* true and false. Broad, E. T. Bell and others, who find the rejection of the law of excluded middle possible and attractive, are thus obliged if our argument is correct, to reject the law of contradiction as well, a result which neither would welcome. In the same way the contention that certain propositions such as: "All numbers of the form $2^{2n+9} + 1$ are factorable" are neither true nor false implies, given the common assumptions, that all propositions of the kind are *both* true and false.

This is one horn of the dilemma. If the law of excluded middle and the law of contradiction are equivalent in any system, inadmissible consequences follow. The other horn of the dilemma is reached if the other course is taken, if the equivalence is denied. In this case, as we have seen, a contradiction develops. Just as Aristotle became involved in a contradiction in so far as he restricted the law of excluded middle (while upholding the law of contradiction and assuming De Morgan's principle and $--p = p$), so modern logicians and philosophers, if they make the same assumptions, are in the same position. The only way

out of this contradiction would appear to be the elaboration of a workable system of logic in which the law of contradiction is retained, while law of excluded middle, and either De Morgan's or $--p = p$ or some alternative principles, is omitted. It might appear that in *Principia Mathematica* the simple omission of one of the three primitive propositions mentioned above (*1.2, *1.3, *1.6) would remove the paradox, since in this case $p \supset p$ could not be proved. But the prospects are not encouraging. If $p \supset p$ could not be proved, some other way would have to be found for making use of the definitions, and this would involve great changes. It would be interesting, however, to have such a system developed. The paradox we have been discussing could be discovered, I think, in most of the logic since Aristotle. It becomes clearer in modern formal systems. In *Principia Mathematica* it has the same status as the paradoxes of implication, such as, for example, the theorem that a false proposition implies all propositions. It does not arise from the formal system itself, but from the formal system in relation to our common understanding and employment of logical operations.

The question now arises whether three-value and multi-value logics do not resolve the paradox. For it is commonly held that there are multi-value logics which assert the law of contradiction and omit the law of excluded middle. If this were so, it would meet the requirements, since the paradox would not appear in a system in which the law of contradiction did not imply the law of excluded middle. Unfortunately, it is extremely doubtful that such a system exists. In the Łukasiewicz-Tarski three-value system, for example, the law of excluded middle does not hold, but neither does the law of contradition. Both are doubtful ($\frac{1}{2}$) when both p and $-p$ are doubtful ($\frac{1}{2}$). Far from resolving the paradox, this three-value system of Łukasiewicz-Tarski seems to add weight to the contention that the two laws, when the usual assumptions are made, are inextricable. In order to retain De Morgan's principle, double negation and other needed propositions, negation, disjunction and conjunction are so defined as to exclude not only the law of excluded middle, but also the law of contradiction. Strictly speaking, of course, it is not these two laws but rather their analogues which fail in the three-value logic.

It would be more correct (but also more cumbersome) to speak of the "analogues" of the principles of the two value logic holding or failing in the three-value logic.

The Heyting logic[4], which gives formal expression to the ideas of Brouwer, appears to come much closer to solving our paradox. For in this system the analogue of the law of contradiction can be proved, while the analogue of the law of excluded middle cannot. It is interesting to observe, however, that this result is only achieved by abandoning the principle of double negation (or rather, its analogue) with the consequence that new paradoxes arise. For example, the system disqualifies not only *some*, but *all* indirect proofs, even the common proofs of plane geometry. (Moreover, there are complications. The Heyting logic needs an unusually long list of primitive propositions, each logical constant must be introduced as a separate primitive idea, and the duality which distinguishes modern logical systems, is much reduced.) Since the truth-value interpretation of Heyting's system leads to paradoxes and there are other interesting interpretations of it which do not, the propriety of this interpretation could be questioned. When it is also remembered that the laws of two-value logic do not occur in Heyting's system, but only their analogues, it appears impossible to hold that this system excludes the law of excluded middle while retaining the law of contradiction. We are therefore obliged to conclude that in no system with which we are acquainted are the two laws independent. In every logic in which the law of contradiction occurs the law of excluded middle is also asserted, while the denial of the latter involves the denial of the former. But, as we have seen, this conclusion contradicts the common view.

III

The equivalence of the law of contradiction and the law of excluded middle (assuming De Morgan's principle and $--p = p$)

[4] A. Heyting, "Die formalen Regeln der intuitionistischen Logik," *Sitzungsberichte der Preussischen Akademie der Wissenschaften*, Physikalish-mathematische Klasse, 1930, pp. 47–58.

Also see Orrin Frink's interesting discussion, "New Algebras of Logic," *American Mathematical Monthly*, Vol. XLV, No. 4, April, 1938.

is of some interest in relation to the dialectic. If these laws are equivalent the restriction of one is as reasonable as the restriction of the other, and dialecticians can scarcely be blamed for rejecting not one, but both. Indeed if one is denied, the other must be denied. It follows, if our reasoning is correct, that the present philosophers and logicians who are ready to restrict the law of excluded middle must be prepared to restrict the law of contradiction as well, or to work out a new system which evades the paradox.

The question whether the law of excluded middle applies universally seems to depend upon the question whether dichotomies occur universally in the world. The law is valid when, and only when, it is possible to find a mathematical point dividing a process of change into *a* and non-*a*, or to make a Dedikind cut dividing the continuum into two segments *a* and *b* such that every element or phase in *a* is less with respect to the quality which is changing than any element in *b* and every element or phase of *b* is greater in the same sense than any element of *a*. That dichotomies or Dedikind cuts are to be found everywhere in society and nature has been questioned or denied by many writers, and there is perhaps no need to repeat their arguments and instances. Professor Dewey in his recent *Logic* puts the position very strongly:

"The notion that propositions are or can be, in and of themselves, such that the principle of excluded middle directly applies is probably the source of more fallacious reasoning in philosophical discourse and in moral and social inquiries than any other one sort of fallacy. . . . An example sometimes put forward to show the meaninglessness of the principle of excluded middle is its inapplicability to existences in process of transition. Since all existences are in process of change it is concluded that the principle is totally inapplicable. For example, of water that is freezing and of ice that is melting, it cannot be said that water is either solid or liquid. To avoid this difficulty by saying that it is either solid, liquid or in a transitional state, is to beg the question at issue: namely, determination of the transitional intermediate state. The objection is wholly sound on any other ground than that the canon expresses a condition *to be* satisfied."

The law of excluded middle, on this interpretation, is not an ontological principle but only heuristic. Since we cannot know

in advance of inquiry that any two propositions, p and $-p$, contradict each other, we cannot know in advance that the law of excluded middle holds for all propositions. And for the same reason we cannot know in advance that the law of contradiction holds for all propositions Professor Dewey concludes accordingly that it is also impossible to interpret the law of contradiction ontologically.

This is what the argument in this paper would lead us to expect. If the two laws are equivalent, granted the usual assumptions, then the one could not lose ontological status without the other losing it too. The status of the two laws would appear to be the same. Both are used as heuristic principles. Both specify conditions *to be* satisfied, and both are sometimes satisfied and sometimes, not. Indeed, if our contention is correct, the conditions which satisfy one law, must also satisfy the other (i.e. when the usual assumptions are made). But it is precisely at this point that it becomes difficult to understand Professor Dewey's account. Although he recognizes instances in which the law of excluded middle does not apply, as, for example, the case of biological species, he fails to give examples where it does apply. Although he recognizes the relevance of the laws to the world, he appears to hold that the conditions they set up *to be* satisfied either cannot be satisfied, or have not up to date been satisfied, in any instance. It is also not clear what the status of the laws would be if the conditions they set up *to be* satisfied *were* satisfied in a given field. Would the laws restricted to this field remain mere guiding or heuristic principles, or would they not also describe and *correspond with* the facts of this field?

One possible view of this matter would be that the laws of contradiction and excluded middle are satisfied when, and only when, a strict dichotomy or Dedikind cut can be set up in the processes of nature and society, and that where this is not possible a certain range of propositions would have to be admitted which are neither true nor false, and both true and false. These propositions could be eliminated, of course, by a definition of "proposition" requiring that they satisfy the laws of logic, but it is doubtful if anything could be gained by a mere change of

name. We should still be in doubt a great deal of the time whether our expressions answer to the customary laws of logic, whatever we call them. In practice, of course, the amenities of polite or routine discourse oblige the assumption that non-Aristotelian propositions do not occur. But when contention or original thinking begins, dichotomies are challenged and the assumption breaks down. The process of denying dichotomies and establishing new dichotomies is a process of clarification. It is subjective or Socratic dialectic which few would question. The customary attitude to objective dialectic, to dialectic as a process in nature and society is utterly different. The idea that dialectical, non-Aristotelian propositions could correctly describe the developmental processes of nature and society is a *bête noire* to many writers who, oddly enough, congratulate themselves on having overcome the Aristotelian view of the world, and who often regard the Aristotelian rules of logic as mere verbal conventions. It would be absurd, of course, to hold that nature imitates our subjective processes of clarification. On the other hand, it seems very arbitrary to deny in advance of inquiry, and in the teeth of much evidence, that sequences of dialectical, non-Aristotelian propositions can describe the transitional states of the objective world. The hypothesis that Aristotelian logic applies in so far as it is possible to set up dichotomies, and that some non-Aristotelian or dialectical logic applies in so far as this is impossible, provides at least a basis for further inquiry.

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Probability and the Theory of Knowledge

BY

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PROFESSOR REICHENBACH'S writings have repeatedly called attention to the important rôle which probability statements play in all inquiry, and he has made amply clear that no philosophy of science can be regarded as adequate which does not square its accounts with the problems of probable inference. Recently he has brought together in convenient form[1] many reflections on the methodology of science familiar to readers of his earlier works, and at the same time he has set himself the task of solving many well-known problems of epistemology in terms of his theory of probability. His latest book is therefore of great interest, both because of the light it throws on Professor Reichenbach's own views and because it reveals the power and limitations of one approach to the problems of science. In particular, while it does not add to the details of his theory of probability worked out elsewhere, the applications Professor Reichenbach now makes of it supply fresh clues for judging its import and adequacy. The object of the present essay, therefore, is to expound a number of his views on probability and epistemology, with a view to examining his conclusions and their relevance to the problems he aims to resolve. The discussion will try to determine whether several features of his present views do not follow from assumptions which he has not

[1] *Experience and Prediction*, Chicago, 1938. All page references, unless otherwise indicated, are to this book.

sufficiently considered; whether his logical constructions do not create new puzzles; and whether a different starting-point should not be taken if the clarification of scientific concepts and procedures, to which Professor Reichenbach's devotion is as unexcelled as it is well known, is to be successfully conducted.

I

At the risk of repeating familiar things, it will be best to begin with a brief exposition of Professor Reichenbach's general views on probability. They are distinguished by three features: his claim that a *univocal* interpretation can be given to all probability statements in terms of limits of relative frequencies; his introduction of the infinitely gradated property *weight* to characterize propositions; and his consequent proposal to replace the allegedly inadequate two-valued logic of truth and falsity by the infinite-valued logic of probability or weights. These features must now be explained in outline.

Professor Reichenbach takes as the basic type of probability statements those with the form illustrated by: "For every correlated pair $x_i y_i$ (x_i being a man undergoing medical examination at a given time and y_i being that man a year later), x_i is tubercular *implies with degree of probability* p that y_i is dead." He maintains that the triadic relation *probability implication* is a generalization of Russell's *formal implication*, illustrated by "For every x, x is diagnosed as tubercular *implies* that x dies within a year." However, Professor Reichenbach interprets the expression "implies with degree of probability p" as follows: Let h_n be the relative frequency in the first n terms of the series with which the y's die when the correlated x's are tubercular, and suppose h_n converge to a limit p as n increases without limit; p is then the degree of the probability implication. Professor Reichenbach believes that such an interpretation can always be given to probability statments, and maintains that it is the only suitable and adequate one. He also declares that probability statements can be formulated so as to express indifferently relations between classes of "events" (or happenings) or between classes of propositions.

It immediately follows that the attribution of a degree of

probability literally to a *single* happening or a *single* proposition is devoid of sense. However, statements which seem to assign probabilities to single happenings do occur and seem to make good sense—as when it is said that it is more probable that Napoleon was ill at the battle of Leipzig than that Caspar Hauser was the son of a prince. Some students have therefore concluded that in such cases a different interpretation must be assigned to the term "probable" than is required on the frequency view, and they have consequently held that the term is *not* univocal. But Professor Reichenbach insists that such a "disparity" view of probability is not necessary. Thus, in the example cited, he regards the difference in probabilities assigned to the two propositions as arising from the difference in the kind of documents which support the two statements; one kind is more reliable because its statements are confirmed with a greater relative frequency than those in the second kind.

For reasons which will be considered presently, no factual statements, according to Professor Reichenbach, can be *known* to be true (or false). Thus, we do not *know* that the next toss with a given die will show a prime number of dots uppermost. Nevertheless, we would be willing to wager on that happening because the probability associated with the *class* of such happenings is high ($\frac{2}{3}$ in this case). This probability, though strictly applicable to classes of happenings (or propositions), may be taken as the measure of the goodness of a wager on a single happening; when so used, the probability functions as the *weight* of the proposition about this single occurrence. Hence the possible numerical values of the weight are the real numbers in the interval 0 to 1 inclusively; and according to Professor Reichenbach, since the truth or falsity of factual propositions can not be determined in practice, the weights of propositions are the practical surrogates of their truth-values. This is a point of fundamental importance in Professor Reichenbach's analysis. He maintains that if we assign weights to propositions (in his sense of "weight"), our acceptance of some propositions and rejection of others "can be justified" because in acting upon propositions with high weights "we shall have in the long run the greatest number of successes." (pg. 310)

Because the determination of the truth or falsity of propositions is not feasible and because we are compelled to operate with them in terms of their weights, Professor Reichenbach concludes that a two-valued logic is not a satisfactory instrument for science; he therefore proposes an infinite-valued logic of probability, of which the ordinary two-valued logic is claimed to be a specialization. For the details of this new logic the reader must be referred to Professor Reichenbach's writings:[2] its elements are no longer single propositions but *propositional-sequences*, each containing an infinite set of propositions and each being associated with a real number in the interval *0* to *1*. Professor Reichenbach maintains that if this number is interpreted as the limit of the relative frequency with which members of the sequence are true, we obtain the usual mathematical calculus of probability; while if it is interpreted as the weight of any of the members of the sequence we obtain what defenders of the "disparity view" call the logical concept of probability. His conclusion is, therefore, that the mathematical and logical concepts of probability are structurally identical, so that the disparity view is demonstrably mistaken (pg. 325). Finally, he develops the idea of probabilities of higher orders, in terms of which he is able to discuss the probability (and so the weight) of probability statements themselves. According to Professor Reichenbach, general no less than singular statements can be characterized in terms of weights, and he therefore argues that scientific theories themselves may be discussed in terms of the frequency theory of probability.

II

For lack of space this paper will not be concerned with most of the details of Professor Reichenbach's probability theory;[3]

[2] Especially *Wahrscheinlichkeitslehre*, Leiden, 1935, See also "Ueber Induktion und Wahrscheinlichkeit", *Erkenntnis*, Bd. V, and "Les fondements logiques du calcul des probabilités", *Annales de l'Institut Henri Poincaré*, T. VII.

[3] For the sake of completeness, however, I add the following summary of some obvious difficulties in Professor Reichenbach's formulation of his system:

a) Probability implication is said to be a generalization of Russell's formal implication. This question has been discussed in some detail by Miss Janina Hosiasson ("La theorie des probabilités est-elle une logique generalisée?", *Actes du Congrès International de Philosophie Scientifique*, Paris, 1935, IV) who points out that in at least three distinct

but it is essential to examine more closely his conception of weight because of the uses to which he puts it in his discussions of epistemology.

It is evident that the weight of a proposition is determined in precisely the same way in which a probability coefficient is obtained. Now probability is defined in terms of the limit of relative frequencies in infinite series; and it will be therefore obvious that the numerical value of a probability can not be calculated by examining a finite number of elements in such series. In other words, no hypothesis as to what is the probability of some designated property can be either conclusively established or conclusively refuted by an inquiry which is necessarily limited to the examination of a finite quantity of evidence. It therefore follows that the weight of a proposition can not be definitely

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senses of "generalization" this is not the case. Will not therefore Professor Reichenbach indicate in precisely what sense of the term does he believe his claim is true? He has promised to reply to this type of criticism, but at the date of writing this reply is unfortunately not available. But however that may be, his formal calculus is obviously not satisfactorily formulated, since he does not specify all the required rules in terms of which the formalism of probability implication is to be treated.

b) Professor Reichenbach has recently admitted (*Philosophy of Science*, Vol. 5, pg. 22) that the many-valued logic as developed in his *Wahrscheinlichkeitslehre* is formulated in the *semantical-language*. He claims nonetheless that the structure of this logic is isomorphic with the structure of the calculus of probability as formulated in the *object-language* of science. But he nowhere gives a proof of this isomorphism, and Dr. C. G. Hempel has recently pointed out ("On the logical form of probability statements", *Erkenntnis*, Bd. VII, pg. 157) that this isomorphism does not in fact obtain. The transition from one formulation of the calculus to the other, is not, as Professor Reichenbach apparently believes, simply a matter of "adding quotation marks".

c) Professor Reichenbach insists that his logic of probability is an "extensional system". The point at issue may be only a verbal one. There are, however, standard definitions of the phrase "extensional logic" in the literature, and according to such definitions his system is *not* extensional. It is not clear what Professor Reichenbach means by saying that "a relation is intensional if it depends on the *intension* of the propositions" (*Philosophy of Science*, Vol. 5, pg. 25); in any case "intension" is not a term which can safely be used in discussing logical foundations without a more careful formulation of its usage than he gives.

d) Professor Reichenbach claims that his probability logic is a "genuine many-valued logic", if propositional-sequences rather than single propositions are taken as its elements. However, the logical connectives between propositional-sequences are introduced in terms of the familiar logical connectives of the two-valued logic (cf. pg. 324). It is therefore not clear why he denies that his many-valued system is built upon a basic two-valued logic in which *propositions* have just two possible truth-values.

established by any finite number of observations. Two questions thus force themselves upon us: 1) By what procedure does Professor Reichenbach propose to assign weights to propositions? 2) In what way is the weight of a proposition a practical surrogate of its truth-value?

1) Professor Reichenbach's answer to the first question requires a discussion of his Principle of Induction and his theory of higher-order probabilities. Suppose then that in order to estimate the probability of getting a head with a coin we toss it n times, and find that the head turns up m times; the relative frequency of heads in this series of tosses is thus $h_n = \frac{m}{n}$. We do not *know* that h_n is the probability of getting a head, but we may "infer" that it is the limit (or close to the limit); in Professor Reichenbach's terminology, in inferring that h_n *is* the probability we make a *posit* or *wager*, without knowing whether the posit is true. We naturally ask, however, what validates this inference; and according to him the "justification" is given by the Principle of Induction, formulated as follows: "For any further prolongation of the series as far as s events ($s > n$), the relative frequency will remain within a small interval around h_n, *i.e.*, we assume the relation $h_n - \epsilon \leq h_s \leq h_n + \epsilon$", with the further proviso that there is such an h_n for every ϵ however small (pg. 340).

It is evident that this Principle is not demonstrable as a principle of logic or pure mathematics, and Professor Reichenbach stands firmly on the ground that it is not apriori either in a rationalistic or a Kantian sense. How then is the Principle itself to be "justified"? His answer is offered as a solution of Hume's problem: We do not know that the Principle is true; but then we do not know that it is false either. Let us therefore make our posits in accordance with the Inductive Rule: if h_n is the relative frequency with which a property occurs in the first n terms of a series, we shall posit that h_s remains within a narrow interval around h_n for every prolongation of the series to s terms.[4] Consequently, if the series has a limit, and if its value is estimated in accordance with this Rule—care being taken to correct the value

[4] *Wahrscheinlichkeitslehre*, pg. 397.

of h as we take more and more terms of the series into account—we have instituted a necessary (though not a sufficient) condition for obtaining the value of the limit; for by taking the latest value of h_n as the best value, we are necessarily approximating more and more closely to the limit if there is one.

Some comments are now in order. a) The above formulation of the Principle of Induction may be misleading if Professor Reichenbach's intent is not made explicit. For example, it is not true that whenever we toss a coin 100 times in 50 of which heads come uppermost, the relative frequency of heads will remain in the interval .50 ±.0001 for every further prolongation of the series of tosses. If the Principle were taken to imply such a consequence it would clearly be false. What Professor Reichenbach means is that for every ϵ *there is an* n such that for every s greater than n, h_s lies in the interval $h_n \pm \epsilon$; but just what the specific value of n for which this is true is left undetermined. It is clear therefore that the Principle asserts nothing more than that the series in question *does* have a limit. b) It follows that even if we make posits in accordance with the Inductive Rule we can not assert, after no matter how many trials, that we *know* what the probability is or, correspondingly, what the weight of a proposition is. No one is more cognizant of this fact than Professor Reichenbach. He points out that in positing a specific h_n as the approximate value of the limit we are making a "blind wager", because we do not in general know *the weight of the proposition which asserts this value.* In proceding according to the Inductive Rule we are therefore employing a self-corrective method, and Professor Reichenbach has performed a valuable service in pointing this out clearly. But does the introduction of that procedure answer the problem which led to its introduction? Are we now in any better position to say what the weight of any specific proposition is?

Professor Reichenbach's further discussion involves the use of higher-order probabilities and what he calls the method of "concatenated induction", with the help of which *blind* wagers are claimed to be converted into *appraised* wagers. This method is said to enable us to "supersede the inductive principle in all those

cases in which it would lead us to a false result, or in which it would lead us too late to the right result" (pg. 364). We must therefore study it in the context of some illustrations of its use which he offers.

Carbon has not yet been liquefied, although most substances can be melted if raised to sufficiently high temperatures. Is it possible, however, to conclude anything about the fusibility of carbon? Letting "A" stand for the fact that a substance is in a liquid state at a certain temperature and "$\bar{A}$" that it is not, Professor Reichenbach represents this state of affairs by the following "probability lattice", in which the letters are ordered from left to right according to increasing temperatures:

Copper	$\bar{A}$	$\bar{A}$	A	A	A.....
Iron	$\bar{A}$	$\bar{A}$	$\bar{A}$	A	A.....
.....					.....
Carbon	$\bar{A}$	$\bar{A}$	$\bar{A}$	$\bar{A}$	$\bar{A}$.....

If an inductive inference is performed from left to right in all but the last line, the conclusion obtained is that the substances in question are in a liquid state when above a sufficiently high temperature; while such an inference performed in the last line yields the conclusion that carbon is not fusible. Each of these inductions is said to be on the "first level". On the other hand, according to Professor Reichenbach, if now an inductive inference is performed in the vertical direction, it leads to the conclusion that carbon will melt at some temperature, because all the other substances do so. This new induction is said to be on the "second level", and Professor Reichenbach declares that "this induction of the second level supersedes an induction of the first level". Each first-level induction is a blind wager, which posits that the limit of the relative frequency of melted states with increase of temperature equals *1* in all but the last row, and posits *0* for the final row. The second-level induction, on the other hand, involves counting the relative frequency with which posits of *1* occur, and itself posits that the probability of posits of 0 is vanishingly small. In brief, the second-level induction appraises the *weight* of each of the blind first-level posits; and according to it

the proposition that carbon is not fusible has a weight close to zero.

Let us examine Professor Reichenbach's argument. a) The object of the outlined procedure is to enable us to estimate the weights of propositions. But as he himself emphasizes, the second-level induction involves a blind wager; and even if we "transform" it into an appraised wager by conducting inductions on higher and higher levels, new blind wagers are inevitably introduced. In what way, therefore, are we any better off *after* carrying through any number of steps of such a theoretically endless process than *before*, as far as determining what the weight of a proposition is? Must we not always be content with an *estimate* of a weight without knowing how *good* an estimate it is—unless procedures and assumptions not made explicit in the above account are introduced?[5]

b) In terms of Professor Reichenbach's procedure, the weight of the proposition "Copper has a melting-point" is high, because of the high relative frequency with which statements of the form "x has a melting-point" are found to hold. It follows that if this relative frequency were small the weight of that proposition would also be small, even if all the evidence relevant to the behavior of *copper* would confirm its having a point of fusion. To

[5] Another example given by Professor Reichenbach helps to bring this out even more clearly. He imagines three urns, containing white and black balls in the ratios $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{3}{4}$ respectively, but with no indication as to which urn contains which proportion. An urn is now selected, a ball is drawn four times with replacement after each drawing, and it is discovered that a white ball has been picked three times. To the question: What is the probability that in drawing balls from this urn a white one appears? the answer given is $\frac{3}{4}$, obtained by using the Inductive Rule. This, however, is a blind wager. Professor Reichenbach converts it into an appraised wager, and declares that the probability that this probability is $\frac{3}{4}$ is equal to 27/46. This last number is a second-level weight, and is said to be greater than the second-level weights for the blind-wagers on $\frac{1}{2}$ or $\frac{1}{4}$. It is evident that this number is obtained with the help of Bayes Theorem, though Professor Reichenbach does not exhibit his method of derivation, on the assumption that the "initial probabilities" for each urn being selected are equal. How does Professor Reichenbach "justify" this assumption? The reader is told no more than that "further posits and posits of the blind type" must be made (pg. 369); and in the more detailed exposition of such "justifications" in the *Wahrscheinlichkeitslehre* (§77), blind posits and new assumptions about the irregularity characterizing hypothetically infinite series are explicitly introduced. The farther one goes along with Professor Reichenbach the more puzzling it becomes why he regards that the problem of determining a weight is solved by him.

take a similar case, Professor Reichenbach would have to say that "Water expands on freezing" has a low weight, because most substances contract on solidifying. But is "weight" defined as Professor Reichenbach defines it and which leads to such consequences, a term for which physicists, or for that matter students of the methodology of physics, can find a useful application? The contrary seems to be the case. Indeed, the grounds we have for believing in the fusibility of carbon are different from those suggested by the above account; they depend upon the theory of matter we accept because of the *weight of evidence* we have for it, and from which the fusibility of carbon is a consequence—it being understood, however, that "the weight of evidence" for the theory is not to be construed in the way Professor Reichenbach understands his term "weight".

c) Professor Reichenbach's predicate "weight" is the back door through which he reintroduces, in so many words, the conception of the probability of a single case, against which—and rightly so—frequentists have always been up in arms. The discussion thus far has tried to show that in order to evaluate the weight of one proposition, Professor Reichenbach is required to evaluate the weights of an infinite series of higher-level statements. But on his premisses, one must also be prepared to evaluate an infinite series of what might be called *lower-level* weights, before a given weight can be determined. For consider an actual case of estimating a probability, for example the probability of a normal coin falling head. We must first count the frequency with which heads fall uppermost in a finite series of throws: we therefore toss a coin on a given occasion, watch it come to rest, note that the head is uppermost, and then continue this process. It would seem, therefore, that we must assert statements like "This coin on this occasion falls heads up". But according to Professor Reichenbach we can not be completely certain of the truth of this statement, since it too possesses a "predictional element"—it possesses for us only a high weight, sufficiently close to *1*, so that we may treat it as true only by schematizing the infinite-valued logic of probability (pg. 327). We are therefore led to ask how this allegedly high weight is to

be determined. The weight Professor Reichenbach has in mind can not be obtained by regarding the proposition in question as a member of the propositional-sequence each element of which has the form "This coin on occasion x falls heads up"—for then its weight would be $\frac{1}{2}$ and the procedure would be circular. Its high weight must therefore accrue to it in virtue of its membership in a different propositional-sequence. But which one? Professor Reichenbach is not very confiding on this general point. *Perhaps* the one he has in mind is the class of propositions each of which is a statement by an observer that after the tossed coin has come to rest the head is noted uppermost; the high weight, close to *1*, of the original statement, would then be the value of a relative frequency in this class. If now we ask how *this* ratio is determined, we must count the frequency with which the elements of this class are "true",—a procedure which once more involves a "schematization", since what we call true is again only a high weight. We are therefore led once more to another propositional-sequence, and then to another, and so *ad infinitum*. Even if the suggested propositional-sequence is not the one Professor Reichenbach has in mind, the conclusions drawn from this example would remain unchanged. Does Professer Reichenbach maintain that his use of the term "weight" formulates what is explicitly or implicitly done in the sciences and every-day inquiry, when we try to "justify" the acceptance of propositions which are to be used for action? Or is he *proposing* that such a procedure be instituted as the only one which would "justify" the acceptance or rejection of propositions? If so, does he think that it is a practicable one?

2) We must therefore turn to the second question—in what way is the weight of a proposition a practical surrogate of its truth-value? The gravamen of Professor Reichenbach's objection to the two-valued logic of truth and falsity is that it is not possible to verify completely and finally a proposition as true or as false. But in order to make clear his position, he distinguishes three senses of "possibility". *Technical* possibility involves the matter of fact human ability at a given time to institute and control certain procedures; *physical* possibility involves compati-

bility with the laws of nature, regardless of human power; *logical* possibility involves no more than sheer logical consistency. Technical possibility does not enter into the discussion of complete verification; on the other hand, while Professor Reichenbach admits that final determination of a truth-value is logically possible, he also believes that the specification of truth-values in terms of such a possibility is not "in agreement with physics" (pg. 42) and should be rejected, because it would permit many sentences to be meaningful which we would normally wish to exclude from being so. What about the physical possibility of complete verification? In the first place, some sentences are obviously not so verifiable, because of the character of the human organism: for example, propositions about a remote future or a remote past, propositions about the internal constitution of the stars, and all universal propositions, including probability statements interpreted in terms of limits of relative frequencies (pgs. 40–2); and in the second place, Professor Reichenbach makes clear that even singular statements about the observable present are not completely verifiable either, because they all involve a "predictional component" so that in the context of the system of statements in which they occur they imply an indefinitely large class of consequences about things not directly observable (pgs. 50 ff., 85). In brief, therefore, it is not possible to determine with finality the truth-value of propositions. This is the reason offered why we must pay attention to properties of propositions other than their truth-value, if we are going to take propositions as a basis for our actions (pg. 66); and Professor Reichenbach maintains that the weight of propositions is just the property which is relevant for our purpose.

He makes it perfectly plain that he regards weight as a property which it is physically possible to determine (pg. 54); and his entire system sinks or swims with this contention. But if the previous discussion does not completely misunderstand him, it is evident that the weight of a proposition is no more determinable than is its truth-value. Professor Reichenbach declares that statements about single events at a definite spatio-temporal position might be supposed to be two-valued, but that as a matter

of fact "it turns out that a determination remains a mere fiction as we never are able to determine the truth-value in question. All that we can do is make an assumption about the truth-value, based on observation".[6] Is not Professor Reichenbach in exactly the same position with respect to the *weight* of such a proposition? Is he not therefore compelled by his own criteria to regard as a "mere fiction" his infinite-valued logic? It may be granted to him, if for no other reason than the argument, that the truth-value of a proposition can not be determined with finality. Will he not also grant that the weight can not be so determined?[7] In any case, there seems no way of escaping the conclusion that *weight*, conceived as Professor Reichenbach does, is no practical surrogate for truth and falsity. He believes that his theory of weights and higher order probabilities solves the problem of the application of the probability calculus to finite classes of elements, if "probability" is defined in terms of limits in infinite series. I would not like to be understood as minimizing the great importance and ingenuity of Professor Reichenbach's contributions to the calculus of probability; but I fail to see why a problem should be considered solved if the proposed solution involves difficulties of increasing complexity and obscurity.

[6] *Philosophy of Science*, Vol. 5, pg. 28.

[7] Professor Reichenbach might reply that although we can not *know* the weight, we can *wager* on it. Surely. But why can not a like reply be made to his charge that we can not know the *truth-value* of a proposition? Professor Reichenbach seems to me not infrequently to confuse the distinction between a proposition having a truth-value and our knowing what this truth-value is. The real trouble in his discussion seems to me to rest on the fact that he has so defined many of his terms that it is not possible to apply them; in particular, "knowledge", "true", and "weight" are so used by him that as a consequence we can never *know* the truth or the weight of any factual statement whatsoever. Are we not doing some violence to the language when we are compelled to say that we never *know* anything? There would be no point in having the term "know" if we consistently used it that way.

Incidentally, Professor Reichenbach explains that while *truth* involves a relation between a proposition and a fact, *weights* are a function of the state of our knowledge and may vary with the latter (pg. 27). But the weight of a proposition, as he defines this term, is as much an "objective" property as truth is, and has nothing to do with the state of our knowledge. Has not Professor Reichenbach slipped into a different conception of weight, at least at this point, from the one he officially avows, and is he not criticizing the two-valued logic from the stand-point of this unofficial view?

III

Is it not relevant to ask, therefore, whether the impasse to which Professor Reichenbach's scheme brings us is not a consequence of his assumption that all statements of probability are to be interpreted in exactly the same way? He is on solid ground when he maintains that many such statements require to be interpreted in the way he indicates. However, he also has his eye on the important fact that *all* statements in empirical inquiry are theoretically corrigible by further inquiry, so that statements accepted as true at one time may subsequently be rejected. But he expresses this fact by saying that "all statements are probable", and does not seriously entertain the possibility that this expression may be equivocal. Against those who maintain that this is in fact the case, he declares that those who favor a "disparity" conception of probability violate commonly accepted "principles of empiricism", in particular the principle that all empirical statements be "verifiable" (pg. 304). This charge, however, is a gross begging of the question; it involves the gratuitous assumption that what some adherents of the "disparity view" call *weight of evidence* or *degree of confirmation* must conform to the same formal conditions of calculation as does probability conceived as a relative frequency (cf. pg. 305)—so that as a consequence the only genuine alternative to his own views would be the theory of Laplace and his successors.

In other words, Professor Reichenbach does not seem to be sensitive to the distinction between the (limit of the) relative frequency with which sentences of a certain form are true in an infinite class of such sentences, and the relative frequency with which certain *consequences* of a sentence are true in a finite class of such consequences. The former yields the frequency interpretation of probability statements, which he favors; the latter does not. Without taking the requisite space to develop this second distinction, which leads to the still insufficiently analyzed notion of degree of confirmation,[8] it is worth while to examine Professor

[8] This point is briefly discussed in *Principles of the Theory of Probability, International Encyclopedia of Unified Science*, Vol. I, No. 6.

Reichenbach's discussion of the problem of induction and the theory of meaning, in order to exhibit the bearing of such a distinction on his conclusions.

1) Professor Reichenbach declares that the aim of induction is "to find series of events whose frequency of occurrence converges toward a limit", even if we have no guaranty that this can be attained (pg. 350).[9] The process of induction will therefore terminate in conclusions which go beyond the known facts, although the regulative principle in the construction of such conclusions must be "the postulate of the best predictive character" (pg. 376). The problem of induction, according to Professor Reichenbach, is to "justify" such conclusions. We must, however, distinguish between the "context of discovery" and the "context of justification". The former involves questions of the psychology and sociology of scientific discovery; the latter, which is alone relevant to the logician of science, "does not regard the processes of thinking in their actual occurrence", and its aim is "to construct justifiable sets of operations which can be intercalated between the starting point and the issue of thought processes, replacing the real intermediate links" (pg. 5). The task of epistemology, and of a theory of induction in particular, is to "replace actual thinking by such operations as are justifiable, that is, as can be demonstrated as valid" (pg. 7). Such a justification is provided by the Principle of Induction, because if we draw conclusions in accordance with it we shall be following "a procedure the continued application of which must lead to success, if success is possible at all" (pg. 377).

One example of the use of this Principle has already been discussed; let us consider another. According to Professor Reichenbach, Galileo's law for freely falling bodies was inferred

[9] This formulation seems to me very dubious indeed, unless it is intended as a nominal definition of "induction". Just what is the series of events for which we are supposed to be searching when we look for the individual who committed a crime? Again, when Kepler studied Tycho Brahe's tables on the positions of Mars, what series of events was he looking for? In this case, certainly, most physicists would agree that Kepler was looking for a *formula* (or law), such that the motion of the planet could be specified by it. It is, however, not essential for the purposes of the present discussion to enter into this point at greater length.

to hold for all cases similar to those in which confirmatory observations had been made; and Kepler's law of areas was also inferred to hold generally from observations on the spatio-temporal positions of Mars. Finally, Newton's gravitational theory, which "includes the observations of both Galileo and Kepler", made it possible to argue for the validity of each law not simply on the basis of the observational material for each law, but on the basis of the joint observational material for either law. "Newton's discovery, therefore, in unifying both theries, involves an increase of certainty for both of them; it links a more comprehensive body of observational material together to form one inductive group" (pg. 372).

Professor Reichenbach does not develop this point further, and it is not easy to see how what he says supports his claim of an "increase of certainty" for the two laws in question. He apparently thinks that because we have made two blind posits on 1 being the limit of relative frequency with which each law is confirmed, and because each law is derivable from Newton's theory, we are in the position to *appraise* these posits. But why is it relevant that the two laws are each derivable from Newton's theory? Why should we not form "one inductive group" with the observations for Galileo's law and the observations for *any other law*, say Coulomb's, for which the same posit is made as to the relative frequency of confirmations? Professor Reichenbach can not say that the laws of Galileo and Coulomb have "nothing in common"—for as he well knows any mathematician can show the contrary. Alternatively, suppose (what is of course not the case historically) that observations on falling bodies and planets had led us to a posit of .80 as the limit of relative frequency with which Galileo's law is confirmed and a posit of .60 for Kepler's law. Why would the fact that each law is derivable from Newton's theory increase the certainty of these inferences? Professor Reichenbach does not explain such matters. He simply assumes that the "justification" of every theory consists in assigning to it a weight, estimated by repeated application of Inductive Rule. This assumption calls for some comments.

a) In the first place, it does not seem at all plausible that

theories, e.g., Galileo's or Newton's, which would be confirmed in say only 80% of their applications would continue to be considered as adequate. It is of course naive to suppose that a single apparent negative instance marks a theory as unsatisfactory; every one knows that a certain range of deviation of observed from expected results is invariably taken into account when testing a theory. There is no such thing as an indubitably final refutation of a theory in the history of science on the basis of isolated negative instances. But having said this, would any physicist retain a theory if a single well-constructed negative instance for it were found—to say nothing of there being an infinity of such exceptions, such a possibility being still compatible with the weight of the theory being *1*? A single negative instance of the kind considered would be a challange to *alter* the theory, to construct and test *alternative* theories, and not to conduct further experiments for the sake of determining what is the limit of the relative frequency of instances confirming the theory in question. On Professor Reichenbach's view every observed value which departs from an expected value, as calculated from a theory, counts as *one* instance, irrespective of whether it falls inside or outside the range of permitted deviations. How can he explain the fact that a single deviation falling outside such a range counts for more, in the judgment of workers in the field, than any number of deviations falling inside the range—especially since the value of the limit of the ratio of confirming instances can not be affected by a finite number of disconfirming ones?

b) In citing the power of Newton's theory to unify and thereby strengthen the evidence for the laws of Kepler and Galileo, Professor Reichenbach has made an important observation. But he does not seem to have interpreted his own illustration adequately, and he has not succeeded in showing how the example supports his theory of induction. It seems to me essential to stress the fact that each of these laws is *derivable* from the Newtonian theory, given suitable initial conditions, so that as a consequence the (necessarily finite) confirmatory evidence for these laws *singly* also confirms the Newtonian theory. On the other hand, we judge the acceptability of the latter in terms of such

factors as the number and variety of the confirmatory instances—in brief, in terms of the "degree" to which the theory has been put to the test. It follows that an observation directly confirming Kepler's law, will also confirm Newton's theory, and will therefore confirm Galileo's law, since an instance confirming a theory also confirms what is derivable from that theory. This account seems to me straightforward though unduly simplified, it involves no *tour de force* such as Professor Reichenbach performs, and expresses what seems to be the procedure of physics; but its cogency depends on not mistaking "probability" as a relative frequency for "probability" as degree of confirmation (i.e., as an indication of the thoroughness with which a proposition has been tested).[10]

c) Professor Reichenbach declares that if we wish to "construct an objective meaning of a term . . . we must ask which meaning we are to ascribe to the term, *if the usual usage of the term is to be justifiable*". He illustrates his point by explaining the meaning of "length of a line" as the ratio of one line to a unit line; such a meaning "if underlaid to the use of the term in the mouth of most people, makes their statements justifiable". Similarly, he claims that if his interpretation is given to all probability statements "all our attitude in respect to scientific theories can be justified".[11] This is a puzzling rule for determining the meaning of terms. I had imagined that in attempting to discover what the term "curvature of space" means in physics, Professor Reichenbach was not aiming at "justifying" the physicist's use of it, but at discovering *how* he does use it. This usage fixes the meaning (or meanings), and it is not easy to see what it would be like to "justify" that usage. Unquestionably, many physicists ascribe a meaning to the term which it does not have; but that it does not have it is discovered by examining how it is used. Assuming such a procedure as basic for discovering the meanings

[10] These matters are explained at somewhat fuller length in the work cited in Note 8. Professor Reichenbach distinguishes between two ways of assigning a probability coefficient to a theory, one of which involves viewing a theory as a logical product (pg. 396). From the point of view of the proposed distinction between probability and degree of confirmation, his complicated and certainly dubious construction is altogether unnecessary.

[11] *Philosophy of Science*, Vol. 5, pg. 33.

of terms, I can not persuade myself that Professor Reichenbach's interpretation of "the probability of a theory" formulates what physicists do with it or even think about it for the most part.[12] Indeed, he himself admits that the probability of theories, in one of the senses he specifies (i.e., probabilities of the second level), is not yet amenable to quantitative determination, because a sufficient body of statistics is not in existence.[13]

Let us, however, consider Professor Reichenbach's one illustration in which a theory is allegedly assigned a numerical weight, in his sense of the term. He translates the statement "The velocity of light is 299796 km/sec. with an average error of ±4, or of ±0.0015 percent" as "The probability that the velocity of light lies between 299792 km/sec. and 299800 km/sec. is $\frac{2}{3}$", and correctly interprets the latter in terms of relative frequencies. He then says that from this we can infer "a lower limit for the probability (on the first level) of Einstein's hypothesis of the constancy of the velocity of light". His conclusion is that "the probability of Einstein's hypothesis is greater than 99.99 per cent, if a numerical range of 0.0052 per cent is admitted for the possible value of the constant" (pg. 398). This is certainly most impressive. But how are these numerical values obtained? I shall overlook "the transition from the special value of the constant to the postulate of constancy in general"[14] which Professor Reichenbach makes, since it raises a question much too technical to discuss here. As for the rest, these numbers are calculated with the help of the mathematical calculus of probability—applied upon sets of numerical measures *obtained under specified conditions of experiment*, when suitable assumptions are made about the distribution of the measures. Professor Reichenbach's figures thus declare that if prescribed types of measurements are carried out, the magnitudes so obtained will lie within a fixed interval around

[12] Professor Reichenbach suggests that those who do not accept his interpretation should ask physicists not what they *mean* by the phrase but what they *do* when they use it (*Philosophy of Science*, Vol. 5, pg. 33). Has anyone really done so on a sufficiently extensive scale? It would certainly be interesting and highly instructive to examine the statistics of the replies obtained.

[13] *Philosophy of Science*, Vol. 5, pg. 41; also pg. 399 of the book under discussion.

[14] *Ibid.* pg. 40.

a certain number with a specified relative frequency. But a study of the procedure by which these figures are obtained reveals nothing relevant for determining the probability of Einstein's hypothesis. It may be that Professor Reichenbach intends his "interpretation" as a *proposal* to the physicist to institute certain practices in evaluating theories; it is most clearly not an account of what the physicist does do.

d) One final point. It is not easy to ascertain what precisely Professor Reichenbach does mean by "justification", or what exactly is the force of "valid" in the passage cited toward the beginning of this section. He says that the construction of the justifying operations is not to be arbitrary, that "it is bound to actual thinking by the postulate of correspondence" (pg. 6); and he also says that the construction of theories must be in accordance with the postulate of "the best predictive character". Now a demand for "justification" is readily understood, if it means that evidence for the theory must be found; such a justification can be given, but only in terms of the consequences or confirmatory instances of a theory and the relative completeness with which a theory has been tested. However, this is not what Professor Reichenbach seems to have in mind, at least it is not the only thing. Thus, he declares that

"it cannot be denied that there is likewise an inductive relation which supports the theory *before* the consequences are tested. The situation of the theory in respect to facts *before the experimental test* is not different, in principle, from that *after the experimental test*. In both cases there are facts which do not *verify* the theory, but which confer a determinate probability on it; this probability may be small before the test, and great after it. But even before the test there must be facts on which the theory is based; and there must be, also before the test, a set of inductive relations leading from the facts to the theory—else the theory could not be seriously maintained. The set of relations before the test may be of a very complicated kind, whereas the relations from the observational material of the test to the theory may be of a rather simple kind; but in both cases they must be of the inductive type if the adoption of the theory is to be justifiable."[15]

[15] *Ibid.* pg. 37; also pg. 382 ff. of the book.

This and similar passages are a puzzle, and it is not altogether clear whether Professor Reichenbach is abiding consistently by his distinction between the context of discovery and that of justification. It is fairly clear, however, that according to him the facts upon which a theory is "based", prior to its being tested experimentally, imply the theory with some degree of probability. Indeed, what distinguishes a great man of science from lesser people is that apparently "he sees the inductive relations" while they do not. But just how are these probability implications to be specified in terms of relative frequencies? Is not Professor Reichenbach flying in the face of our best attested experience as to the historical genesis of theories? Is it at all plausible to believe that "the facts" antecedently to the theory being tested can do more than *suggest* the theory but without determining its specific form? And incidentally, if the alleged probability implication between the facts and theory is a relation involving limits of relative frequencies, how can the probability of a theory be radically altered, as Professor Reichenbach admits it may, by the addition of a *finite* number of confirming instances? On the other hand, many of his remarks about the probability of theories become statements of accredited methodological principles, if we interpret them to be about degrees of confirmation. Thus, to "justify" a theory will mean that an appropriate degree of confirmation is to be found, by marshalling a certain finite quantity of evidence to support it; to say that a theory has a "probability" before it is experimentally tested will mean that the "known facts" confirm the theory to some degree, because the propositions asserting them are connected with the theory by a chain of derivations; to maintain that the "probability of a theory" may be made greater by an experimental test will mean, for example, that an experiment, carried on in subject-matter qualitatively different from that in which support has already been found for the theory, will increase the degree of confirmation. Should not Professor Reichenbach therefore give some thought to the notion of degree of confirmation? Will he not contribute his great talents in order to clarify it more fully?

2) The consideration of Professor Reichenbach's discussion of

meaning must be brief. The theory of meaning is central to empirically oriented philosophies which take their cue from the procedures of the natural sciences; and at least since Peirce, they have been occupied with setting up criteria for distinguishing meaningful statements from those which are not. It is one of the merits of Professor Reichenbach's discussion that he recognizes that the setting up of such criteria is primarily a matter of making *decisions*, involving questions not of truth and falsity in the first instance, but questions of the practical consequences which follow from making them.

Accepting by and large the standpoint of those who believe there is some connection between the meaning and the verifiability of propositions, Professor Reichenbach first considers what he calls "the truth-theory" of meaning. According to this view a proposition is meaningful if and only if it is completely verifiable as true or false. He points out that such a criterion is very inconvenient, because it is not physically possible to determine the truth-values of most statements with finality; hence such a decision would require us to dismiss as meaningless most of the statements of science. He accordingly proposes a different criterion, which yields "the probability conception of meaning" and is formulated in terms of the weight of propositions instead of their usual truth-values. On this criterion a proposition has meaning if and only if it is physically possible to determine for it a weight; and two propositions have the same meaning if and only if they obtain the same weight by every possible observation (pg. 54).

Is this a suitable criterion? The preceding discussion has shown that the weight of a proposition can be determined with no greater success than its truth-value. If follows that Professor Reichenbach's probability conception of meaning involves the same undesirable consequences as does the truth-theory of meaning. Consequently, if we do not wish to classify as meaningless the statements employed in the sciences, the first part of his criterion must be judged as inappropriate.

Waiving this point, however, we may next ask whether the second part of the criterion concerning the sameness of meaning

is adequate for the purposes Professor Reichenbach has in mind. Now suppose that we are about to toss a normal die and that we pronounce the following two statements: "This die will fall with the one-spot uppermost on the next toss", and "This die will fall with the five-spot uppermost on the next toss". Everyone would agree that these statements predict different things, and that no criterion of meaning can be regarded as satisfactory which fails to distinguish between them. But according to the way Professor Reichenbach assigns weights to such propositions, each of them has the weight $\frac{1}{6}$, because each is a member of a propositional-sequence in which by hypothesis $\frac{1}{6}$ is the limit of the relative frequency with which propositions having a specified form are "true". All possible observations will ascribe the same weight to them, and they should therefore have the same meaning. The proposed criterion for sameness of meaning thus seems to be inadequate.

On the other hand, at least a part of Professor Reichenbach's criterion could be saved, if in his formulation "weight" were interpreted as the degree of confirmation for a proposition. For what is required of statements in the sciences is that from them, with the help of the techniques of logic and in conjunction with other propositions stating suitable initial conditions, *other* statements be derivable,—so that at least some of these derived propositions can be controlled by further observations. In brief, what is required is that statements be *confirmable*, either positively or negatively, by appropriate observations. Accordingly, a suitable criterion of meaning would seem to be that a proposition be confirmable, or alternatively that it be physically possible to assign a degree of confirmation for the proposition in question. However, it is not clear how a criterion for sameness of meaning is to be formulated in terms of "degrees of confirmation", even if we assume that such "degrees" are always comparable. What we should want to say is that two propositions have the same meaning if all propositions derivable from them which can be directly controlled by observation, receive the *same* confirmation —i.e., such derived propositions are either all positively confirmed or all negatively confirmed. But these speculations are obviously

hints for further discussion, and are introduced only for the sake of suggesting that the notion of weight of evidence or degree of confirmation seems to be a fruitful though vague idea, well worth considering as a promising alternative to Professor Reichenbach's conception of weight.

IV

I now turn to Professor Reichenbach's analyses of issues with a distinctively traditional epistemological flavor. In so far as his subsequent discussion depends on his theory of probability, the epistemology he offers falls under the judgment already made upon that theory. It is worth while, however, to consider his epistemological reconstruction of the world on its own merits, even if it should raise old issues of probability theory in a new dress.

1) Professor Reichenbach is not one of those thinkers who dismiss the problem of the external world as meaningless, and not only does he take much pains to defend it as genuine, but he offers a definite resolution of it. What is the problem, according to him? The problem is generated, as always, by the search for *direct* propositions having absolute certainty. Propositions about "immediately observable facts" (i.e., such "concreta" as chairs and tables) turn out to be not completely verifiable, because they involve a predictive element and so are *indirect* and not absolutely certain. But, so the argument runs, in order to confirm, at least in part, such indirect propositions we must finally reach propositions which *are* direct. The view Professor Reichenbach first considers takes these direct propositions to be about *impressions*. Impressions (a term which Professor Reichenbach takes to be synonymous with "presentations", "sensations", and "sense-data") are described as "phenomena occurring within my mind but produced by physical things outside my mind" (pg. 89); impressions are "events of my personal sphere" as distinct from "the external world", they do not have "an independent existence", and in observing them "I observe only the impressions produced by" external things on my private world. Since by hypothesis impression statements alone are

absolutely certain, the problem to be discussed is "How do we know that there is such an external world outside our private world?" (pg. 90). Professor Reichenbach subsequently rejects this hypothesis as to the indubitability of impression statements. He grants it meanwhile for the sake of the argument, maintains that the problem is a serious one, and claims to be able to solve it with the help of his theory of probability. I shall first expound his alleged solution, then argue that it falls short of being one, and finally claim that the "problem" itself is not intelligible.

a) Professor Reichenbach's discussion of the problem is carried on largely in terms of the following analogy. Imagine a cubical world with translucent but not transparent sides, and that its inhabitants note changing shadows upon the walls and ceiling—shadows which *we* know are those of birds flying outside the cube but which are not observable to those in the interior. These shadows stand in the same relation to the external birds as do the private impression to the physical things producing them (pg. 115); and the problem to be considered is whether the inhabitants could ever discover that there are things outside their cube. Professor Reichenbach next supposes that some interior Copernicus finally establishes correspondences between shadows on the walls and ceiling, and that he explains these relations by the theory that the moving shapes are shadows of birds existing outside the cube. He also supposes that certain posivistic philosophers reject this hypothesis, declaring it to be a pseudo-statement because the birds are not accessible to observation, and maintaining therefore that the meaning of every statement about the birds is the same as the meaning of sets of statements about the observable shadows. Professor Reichenbach finally adjudicates between the disputants. He declares that *if* the probability-conception of meaning is accepted, Copernicus is in the right: for his theory has a "surplus meaning", and the weight of the bird-hypothesis is higher than the weight of the mere-shadow-hypothesis. He admits "that the observable facts do not furnish a difference as to the absolute truth or absolute falsehood of the two theories in question; but the weight conferred on them by the facts observable within the cube is different" (pg. 121); and he

also declares that "what is remarkable here is that the two theories obtain, from the facts observed within the cube, different weights, although both theories furnish for future facts within the cube the same weights . . ." (pg. 124). Finally, he insists that

> "Our situation with regard to external things is not essentially different from that of the inhabitants of the cubical world with respect to the birds outside: imagine the surface surrounding that world to contract until it surrounds only our own body, until it finally and with some geometrical deformation, becomes identical with the surface of our body,—we arrive then at the actual conditions for the construction of human knowledge, all our information about the world being bound to the traces which causal processes project from the external things to the surface of our body" (pg. 154).

Accordingly, the problem of the external world is solved in the same manner and with the same conclusion as is the predicament of the cube-inhabitants: the external world has an existence of its own, independent of our impressions (pg. 129 ff.).

b) Professor Reichenbach's solution hangs upon the validity of his claim that the bird-hypthesis has a higher weight than the mere-shadow theory. Unfortunately, he does not explain how this higher weight can be established, and the only clue he offers is a reference to Bayes Theorem in the calculus of probability. He declares that the "backward probabilities" from the observed facts to the two theories are different, even though the "forward probabilities" from each theory to the facts are the same, because of the difference in the *initial* probabilities of the two theories (pg. 124). The argument thus depends on the possibility of assigning probabilities to *theories*, in his sense of the phrase; and reasons have already been advanced why this is not feasible. Waiving this point, however, let us ask how these initial probabilities are to be determined. Now Professor Reichenbach is unquestionably a frequentist in his interpretation of "probability", and these initial probabilities must therefore be estimated by counting frequencies in material *accessible to those within the cube.* But what sort of frequencies observable within the cube would be relevant to determine the weight of

a theory about things not accessible? Professor Reichenbach does not explain this, and lacking such an explanation we might rest our case as to his not having solved the problem until he does so. Let us imagine, however, what his explanation *might* be, though we have no assurance that we read his intent correctly. He might suggest, to put it quite crudely, that the inhabitants would count how frequently moving figures on a screen are shadows of objects behind or before a screen, that the relative frequency with which this is the case is high, and that therefore by an obvious argument from analogy the bird-hypothesis would have a high weight. But on reflection it becomes apparent that the extrapolation from observed frequencies *within* the cube to events inaccessibly *outside* the cube, assumes the very point at issue: the existence of an *outside* to the cube could not be in question if the argument is to mean anything. For what else is involved in the supposition that the inhabitants observe shadows on the *walls* of their world? What would it mean to say that they are hemmed in by "a wall", if the wall did not have an outside as well as an inside? Having assumed so much of a world outside their cube, just what is the problem of the external world to the inhabitants? But without such an assumption, how can the cube-Copernicus, or Professor Reichenbach, even *formulate* the problem? Imagine some terrestrial "physicist" who propounds a theory according to which the familiar objects of common-experience such as tables and chairs, are simply "projections" into our three-dimensional space from things existing "outside" in an inaccessible four-dimensional one. What would Professor Reichenbach's reply be to such a hypothetical physicist who repeated every one of his own arguments for the existence of an external world to establish the existence of such an "external" fourth-dimension? Would he not say that such a "theory" can be intelligibly formulated only if "being external to our space" were given a sense which makes that outside accessible?

Professor Reichenbach's argument does not become easier to understand if we apply it to the problem of inferring an external world from the "personal sphere" of impressions. For just how

is one to construe the inevitable task of estimating the initial probability of the hypothesis of an external world by counting frequencies *within* the surface of the human body? I am forced to conclude, until such time as Professor Reichenbach explains such matters, that he has not solved the problem he set himself. Moreover, I wish to indicate briefly that he has not even got a problem to solve. Impressions are explained to be observable events "of my personal sphere"; and Professor Reichenbach even says that knocking a man's head against a wall is not a good way of persuading him of the reality of the wall, because what the man saw was "outside his body" whereas the pain felt was "inside", so that his problem as to whether there is something "outside of himself" is not thereby answered (pg. 92). He does not even hesitate to talk of impressions "being produced" by external things on the "surface of our body". I have not been able to locate any account by him of impressions which does not employ such terms. But if this language is taken seriously, the formulation of the problem of the external world involves the assumption of the existence of such a world. For how otherwise can a distinction be defined and maintained between inside and outside, mine and not mine, on the surface and beneath or above the surface, produced and simply appearing, observed and not observed? An impression cannot be identified as "private" except in the context of things which are *not* private; and a datum of experience cannot be judged as directly observed except in reference to an organism which is not such a datum, and whose introduction therefore assumes the external world the existence of which was allegedly put in question. Without the use of such distinction, however, how can the problem be formulated?[16]

[16] Recently Professor E. J. Nelson has formulated the problem in somewhat different language. "By an external world I mean any particular or system of particulars, of whatever nature . . . which is not immediately experiencible. . . . The problem before us then is this: Is it possible to construct a valid inductive argument which concludes from phenomenal evidence or data of experience to the existence of an external world". *Philosophy of Science*, Vol. 3, pg. 238. He does not say in what sense of "ible" the external world is not to be immediately experiencible, and his question as it stands is therefore not sufficiently explicit. But in any case, what is to be understood by "immediately"? Does it connote a definite interval of time? If not, how does one go about determining whether anything is immediately experiencible? If so, must not the determination of whether

2) Professor Reichenbach's discussion of the problem of the external world is a *tour de force*, almost by his own confession, and serves merely to exhibit the power of his theory of probability. For the discussion was generated by the assumption that impression propositions are absolutely certain. But Professor Reichenbach denies that they have this indubitable character, and considers in detail their more dubious epistemic status. We must examine what he says about them, for his account is by no means clear.

a) His denial that impression propositions are absolutely certain is predicated upon his denial that "impressions have the character of observeable facts". He declares that although he has observed such concreta as tables and houses, he has never seen his "impressions of these things"; and to elucidate this point he adds that "I hear tones . . . but I do not hear my hearing of them . . . I feel heat and cold . . . but I do not feel my feeling them" (pg. 164). His conclusion is that the existence of impressions is *inferred*, so that impression propositions are *indirect*.

Before expounding his analysis of the chains of inferences involved, we must pause to overcome our surprise at these pronouncements. Professor Reichenbach discussed the problem of the external world on the assumption that impressions *are* observed, for without it the nature of that problem grows even more mysterious. Indeed, the shadows on the walls of the cubical world, which was offered as a precise large-scale model of the epistemological predicament, must certainly be *observed* if there is to be any point to that discussion. His denial just cited therefore indicates some obscurity in his use of the word "impression". On the one hand, he equates it to such terms as "sense-data" and "representations" (pg. 165), and because of the use he makes of it in discussing the problem of the external world it is natural to suppose that it refers to *what* is seen, heard,

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something is immediately experiencible or experienced involve *other* times with respect to which the immediate is defined? And how does Professor Nelson determine whether something is *phenomenal* except in terms of the mechanism of the human body and therefore in terms of something which does not function in that determination as a datum of experience? What then is the problem of the external world, if in order to state it that world seems to be implicitly assumed?

or felt.[17] So used it is a bit strange to say that impressions are *inferred*, because while empirical evidence supports the claim that colors, sounds, pains, etc., are not psychological primitives, they are nonetheless *not inferred* but are *selected and isolated* in analyses undertaken for definite ends. On the other hand, in the present context Professor Reichenbach is using the term "impressions" to refer, not to what is observed, but to the *act or internal process* of observing. Indeed, he says "An optical sensation is not observed by a man who sees things outside his body; it is inferred. The man *sees* a thing before him and *has* a sensation. . . . He does not know anything about its qualities, except that it has a certain correspondence to the immediate thing he observes. It is an unknown" (pg. 237; cf. also pgs. 167, 172). But if this interpretation is consistently given to the term, just what is the meaning of the "problem" of the external world and Professor Reichenbach's "solution" of it? And just what is the point of his long argument that impressions, in the second sense indicated, are not observed, and that propositions about them are not indubitable—since impressions in this sense are on par with any objects or processes discussed in physics?

b) Let us, however, resume the exposition. If impressions are not "given", what is given? According to Professor Reichenbach what is given are things or states of things (including certain states of our own bodies); and propositions about them are observation statements with the maximum of directness. From these observation statements we then infer the indirect propositions of physics on the one hand (these are about such things as atoms, electric currents, and the like), and the indirect impression statements on the other (pg. 169). The indirect propositions

[17] In discussing the problem of the knowledge of other minds, Professor Reichenbach says: "Other people tell us that they also see the red and feel the heat and taste the sweet; but we never can compare these *sensations* with ours, and so we do not know whether they are the same"; and again, "It is in a certain sense true that *impressions* of different persons cannot be directly compared. Imagine a man who sees green when I see red, and red when I see green—would we ever know this? A mind untrained in philosophy might perhaps object that the man in question would be in permanent conflict with the traffic regulations when driving a motorcar, that he would cross the street at the red light and stop on the green light—but of course this is thoroughly false" (pg. 248, italics not in the text). Here the sense of the passage requires that the italicized words refer to *what* is observed.

of physics have a weight inferior to the observation statements, because the former have a "surplus meaning" when compared with the latter. But the impression statements, although indirect, have a weight superior to the observation statements, because the former have "less meaning" than the latter (pg. 170). Nevertheless, the weights of impression statements are also less than one.

These conclusions about impression statements are certainly curious, and Professor Reichenbach's argument for them are no less so. He first claims that impressions must be "described" in terms of physical objects, and that the descriptions take the form of disjunctive propositions. However, in order to obviate the need for enumerating the terms of such disjunctions he introduces the concept of *immediate similarity*. Thus, the table I observe at one time may be *similar* to the table I observe five minutes later "as far as it concerns only what I see just now", irrespective of whether there is a physical identity between the "two tables" (pg. 170). Consequently, an "impression of a flash of lightning" can be described by the statement "I had an impression produced by the beam of a light house, or by another physical object which stands in the relation of similarity to such a searchlight". More generally, Professor Reichenbach constructs certain statements he calls *basic*. One kind, called *the longer similarity disjunction*, has the form: "There is the thing *a*, or a thing similar to *a*, or there is no observed physical thing, but only an impression as it would have been produced by the thing *a*"; it may be expressed in the self-explanatory notation: a or $S'(a)$ or $I'(a)$. A second kind of basic statement, called *the impression form*, differs from the preceding in that an "impression term" also occurs in the first two members of the disjunction; it may be expressed in the notation: $[a$ and $I'(a)]$ or $[S'(a)$ and $I'(a)]$ or $I'(a)$. Omitting some inessentials, Professor Reichenbach's argument is as follows: The weight of the longer similarity disjunction is greater than the weight of its first member, by a theorem in the calculus of probability. Secondly, the weight of the impression form is smaller than the weight of the longer similarity disjunction, again by the calculus;

nevertheless, "we may conceive it as highly probable that there is always within myself an internal process when I see a thing, and so the weight of the impression disjunction is not much less than the weight of the longer similarity disjunction" (pg. 174). Next, by a theorem of formal logic the impression form is equipollent with its own last member. Consequently, the weight of "$I'(a)$" is greater than the weight of "a"; that is, the weight of the observation statement "There is the thing a" is *smaller* than the weight of the impression statement "I have an impression of the type produced by a". However, basic statements require a comparison between a present object and a formerly observed one, so that they involve the reliability of memory which is fallible; it therefore follows that impression statements are not absolutely certain (pg. 176).

It is difficult to suppose that in this discussion of impression statements Professor Reichenbach is not shuttling back and forth between the two senses of "impressions" indicated above. Can we readily imagine any one asserting "I have the impression of a flash of lightning" if he did not intend to refer to *what* he sees but to indicate the occurrence of a physiological process under his skin? On the other hand, how can we understand the reason for this complicated argument unless Professor Reichenbach intends such a statement to be about something *not* observed but inferred? But however that may be, his argument depends on some theorems in the calculus of probability, in particular the theorem that the probability of two alternatives is in general greater than the probability of either alternative. Now it is evident that such comparisons can be instituted only if it is assumed that the magnitudes to be compared exist. Professor Reichenbach assumes that weights can be assigned to his longer similarity disjunctions and therefore to impression statements. But how is one to go about estimating the weight for such a statement as "There is only an impression as it would have been produced by a flash of lightning" (pg. 178), where by "impression" is understood either an inner process or something seen? What is the propositional-sequence in which such a statement is an element, and how are the relative frequencies

within it to be determined? Professor Reichenbach offers no help to his puzzled readers, and produces not one shred of empirical evidence for his contention. Does he really find it plausible that the weight of an observation statement about the coincidence of a pointer and a scale-reading is less than the weight of a proposition about some inner process alleged to accompany such observations? These inner processes are presumably physiological (cf. pg. 180), and must therefore be investigated in a way continuous with the way we study the behavior of men in China or the structure of binary stars. Is it all reasonable to assign a privileged epistemic position to propositions about such inner processes? In many inquiries it is usual to isolate certain events such as pointer coincidences, and make them the basis for further inferences because of the reliability of our judgments about them; but on Professor Reichenbach's account, such a standard procedure of the sciences is quite inexplicable. Again, why does it follow from the fact that memory is in general not infallible, that some *particular* basic statement has a weight different from *1*? Professor Reichenbach does not say, and an enormous gap yawns at this point in his argument. His discussion of impression statements, like his handling of the problem of the external world, thus seems to be primarily an exercise in the calculus of probability, and sheds little light on the concrete procedures of inquiry.

V

One final portion of Professor Reichenbach's epistomology remains to be considered. The preceding discussions, according to him, have established the following points: that no empirical statements are absolutely verifiable, that to each of them a weight can be attached which is in general less than *1*, and that the observation statements upon which we base all our knowledge are about *concreta*, i.e., "the concrete things of daily life" (pg. 195). The remaining constructive task which he sets himself is to exhibit the structure of the inferences from such a basis to the various complex objects of physics and physiology. We shall follow him only over part of his argument, but over a sufficient portion to come to the heart of his epistemology.

a) Although Professor Reichenbach asserts that the basic observation statements are about the concrete things of daily, he rapidly disconcerts his readers by first offering a "logical construction" for such objects in terms of material allegedly more primitive psychologically. He begins the discussion by distinguishing between different "kinds of existence".[18] Thus, when at dusk a bush is mistaken for a man, *what* we see at the moment we see it has *immediate existence*; and when we subsequently discover that it is no man but a bush, what we then see also has immediate existence. On the other hand, what is seen in the first instance (i.e., the man) has *subjective existence* only, while what is seen in the second instance (i.e., the bush) has *objective existence.*

However, the determination as to whether what is observed has subjective or objective existence is not a matter for immediate intuition, because objective existence is "a determinate logical function of subjective existence" (pg. 203). How is this distinction established? According to Professor Reichenbach, the world of immediately existing things is the world of concrete objects around us, "entering into our knowledge without any intellectual operations being performed by us". In this immediate world, said to be historically and psychologically "original" or primitive, there is no difference between waking and dreaming, and "everything exists exactly in the form in which it is observed"; indeed, whenever doubts concerning the *physical* world occur "we go back to the immediately existing objects, to the concreta, as the most reliable facts" (pg. 204). On the other hand, we draw inferences from (propositions about) immediately existing things to other such things, having the form "If so-and-so, then such-and-such", some of which are successful while others are not. Accordingly, things immediately existing which do not have the consequences they "normally" have are classified as merely subjective existents—where normality is specified statistically in terms of a high ratio of successful predictions made on the basis of a certain class of immediate things (i.e., those

[18] Professor Reichenbach explains that he will use the term "existence" as in modern logic in connection with *descriptions* not things or individuals; but as will be evident he does not abide by this rule consistently.

which are then characterized as having objective existence) (pg. 207). Since the property of being an objective existent is thus specified statistically, we can never be absolutely certain that something immediately existing is objective. Nevertheless, the weight of propositions about the concrete things of daily life is quite high as a matter of fact; most of the concreta of daily life are "for us, real beyond every doubt, because they have stood up to every test ever applied" (pg. 210).

The point of these remarks therefore seems to be that propositions about common-sense objects like chairs and tables, to which objective existence is generally attributed, are themselves "logical constructions" out of propositions about immediate existing things. Such "objects" cannot be judged to be objective existents simply from the fact that something exists immediately. In other words, Professor Reichenbach seems to be subscribing to the sound thesis that predication of "objectivity" is a case of *knowledge* but that the immediate existence or presentation of anything is *not*; and there is much in his book which supports his taking such a stand. Nevertheless, Professor Reichenbach's position is not unambiguous. Note, for example, his reference to immediate existing objects as the most *reliable* facts, suggesting that immediate existents are things known or to be known. Note again how simply a problem of knowledge is apparently generated when we ask what the *concreta* are upon which the world is to be constructed as a basis. These concreta are evidently *not* the common-sense objects, the chairs, tables, houses, and thermometers, for as common-sense objects they are already *objective*, and Professor Reichenbach's aim is to "logically construct" them out of more primitive stuff. If, on the other hand, the concreta are immediate existents with a lifetime only during the moments we observe them (cf. pg. 199), how can *they* have stood up under *repeated* tests since as immediate existents they are by hypothesis incapable of repetition? And have not the common-sense objects in fact become inaccessible to observation and knowledge, now that the immediately existing things (which Professor Reichenbach will presently tell us are only subjectively existing) are taken to be the appro-

priate objects of direct knowledge? There is ground for at least suspicion that Professor Reichenbach has on his hand the riddles of a presentational theory of knowledge—suspicions which are permitted to ripen fully in his discussion of scientific objects.

b) According to him, *illata* (i.e., scientific objects such as atoms, electrons, radio waves, and the like) are not concreta because for physical reasons they cannot acquire immediate existence, although it is not *logically* impossible that we should see radio waves "as we see those of light" (pg. 215). Nevertheless, we can infer their objective existence as "independent entities", though of course only with probability. Professor Reichenbach's discussion of the relations between illata and conreta must be cited at some length. He first explains that the "concreteness" sometimes attributed to things is mistakenly taken to be restricted to "things of a material existence", because what things can enter the domain of immediate existence is "not at all determined by the place of things in the physical arrangement of the world, but by psychological conditions". He then declares that the perspective in which we see the world from the standpoint of our "middle-scale dimensions" is one-sided.

"We see the world in the scale of our sense capacities: we see houses, trees, men, tools, tables, solids, liquids, waves, fields, woods, and the whole covered by the vault of the heavens. This perspective, however, is not only one-sided; it is false, in a certain sense. Even the concreta, the things which we believe we see as they are, are objectively of shapes other than we see them. We see the polished surface of our table as a smooth plane; but we know that it is a network of atoms with interstices much larger than the mass particles, and the microscope already shows not the atoms but the fact that the apparent smoothness is not better than the 'smoothness' of the peel of a shriveled apple. We see the iron stove before us as a model of rigidity, solidity, immovability; but we know that its particles perform a violent dance, and that it resembles a swarm of dancing gnats more than the picture of solidity we attribute to it. . . . We do not see the things, not even the concreta, as they objectively are but in a distorted form; we see a *substitute world*—not the world as it is, objectively speaking.

"Using the terminology developed above, we should say that even

the concreta are only subjective things, of the type to which an objective thing of different form is coordinated. These things are coupled, but they are not strictly speaking identical. . . . Our immediate world is, strictly speaking, subjective throughout; it is a substitute world in which we live." (pgs. 219–220).

What is the ground offered for these most important conclusions? The reason is that the mechanism of sensation is so organized that it cannot produce a sensation without superimposing upon it a certain "description."

"We do not see things as amorphous but always as framed within a certain description. . . . The objects of our sensations always have a '*Gestalt* character'. They appear as if pressed into a certain conceptual frame; it is their being seen within this frame which we call immediate existence. The description in whose frame we see things corresponds to the objective thing only to a certain extent . . . [and] is never more than a substitute for a completely true description and will express only certain more or less essential features of the physical object." (pg. 221).

The argument has now completed its full period and the bearings of Professor Reichenbach's epistemological principles are apparent. Let us first clear away minor details. Professor Reichenbach began by claiming that the world of immediate existents enters "into our world without any intellectual operations being performed by us". He has now apparently gone back on his word, maintaining that even immediately existing things are overlaid by a descriptional framework which we introduce; the alleged "subjectivity" of what is observed is taken to follow from this fact. On the other hand, he also maintains that the descriptions in whose frame we "see" things correspond to the objective things only to a certain extent. Now how does Professor Reichenbach know this? What avenue of information has he got about the objective world which enables him to characterize our judgments about the observeable world as "false in a certain sense"? For is not the reason for his discrediting judgments of observation that they are conditioned by the mechanism of the body? Is the knowledge he has of the objective things *independent* of such mechanism—is not this

knowledge *also* selective and formulated in a "descriptional frame"? Why should not the knowledge he claims to have of scientific objects be also characterized as not being of objective things, for the very reasons he advances for ruling out the objectivity of shapes and sounds and textures?

Professor Reichenbach also declares that the table *seen* as smooth is not *objectively* smooth. But if the polished table-top is not objectively smooth, to what *can* the term "objectively smooth" be legitimately applied? How would he characterize that table? Surely it is in consonance with established usage to say, and therefore in accordance with the meaning of the phrase, that a polished table-top *is* objectively smooth. If Professor Reichenbach decides that such a table does not have this property, is he not simply *altering* the accepted meaning of the expression, and substituting for it a sense which is different from that ordinarily put upon it, and therefore irrelevant to the specific context he is considering?

There is much in his discussion which suggests that according to him *all* qualities whatsoever have only "subjective existence". Thus, in enumerating the various things that can directly appear as immediate existents he mentions "things, or states of things, including states of my body", but maintains that qualities like red, warm, and hard are *not* given because "they do not belong to the external things"; and he also maintains that science has shown that things have such qualities only when they enter into relations with our own bodies (pg. 168). Again, he declares that the fact that we see parallel railroad tracks as convergent "is a subjective fact, since the objective physical stimulus does not give any indication as to this psychological fact" (pg. 242). But it is not clear, however, how it follows from the fact that the occurrence of certain qualities is *conditional* upon the occurrence of certain physical processes, that those qualities have only subjective existence. Does not Professor Reichenbach find something curious in an argument which uses admitted facts of human physiology to support the conclusion that properties observeable in physiological processes have only subjective existence? Again, is not the convergence of the parallel rails

an objective fact about the rails, the behavior of light being what it is? Or is it Professor Reichenbach's view that an account of the rails in terms of mechanical properties is a "truer" account than one in terms of their optical ones? There is some ground for suspicion that he has simply baptized as "subjective" whatever has *conditions* for its existence, occurrence, or appearance. In such terms, however, what thing or what property of things can rightfully claim objective existence?

Professor Reichenbach's gravest error seems to me to consist in his taking perception as a case of knowledge, contrary to the main intent of his own argument. To prevent the possibility of misjudging him, I shall add one further citation to those already given. In explaining that in spite of the subjectivity of concreta we need not therefore renounce the possibility of ever obtaining a "true picture of the world", he declares:

"The perspective of the beetle in the meadow is better than ours in the sense that it allows a more precise observation of the individual blades; but the green evenness of the meadow which *we* see is an essential feature also, although unattainable for the beetle. When we see the polished table as a smooth surface, this is not simply false—this picture contains some qualities of the physical table which the picture of the swarm of gnats suppresses, namely, the relative smallness of the corpuscles and interstices compared with the two-dimensional extension. It is true that our substitute world is one-sided; but at least it shows us some essential features of the world. Scientific investigation adds many new features. . . . It is our task to organize all the different pictures obtained in this way into one superior whole. Though this whole is not, in itself, a picture in the sense of a direct perspective, it may be called intuitive in a more indirect sense. We wander through the world, from perspective to perspective, carrying our own subjective horizon with us; it is by a kind of intellectual integration of subjective views that we succeed in constructing a total view of the world, the consistent expansion of which entitles us to ever increasing claims of objectivity." (pg. 225).

He also explains that a logical construction of the world on the basis of immediate existents "allows us to obtain basic statements of a high degree of certainty; for it is much more certain that there is an immediate thing *A* than that there is an objective

thing *A*" (pg. 266). Indeed, apparently forgetting what he said previously about impression statements, he declares that the highest initial weights are assigned to propositions about immediately observed things, so that "the most certain knowledge is consequently of the immediate present" (pg. 281).

I frankly do not understand Professor Reichenbach if he does not mean to say that the beetle in *perceiving* blades of grass is having *knowledge* of the grass, just as we are having knowledge of it in perceiving the green of the meadow, even though both "views" are subjective. I do not pretend to know what can be meant by having certain knowledge of the immediate present; but I think it is clear that Professor Reichenbach claims such knowledge, and that in making such a claim he is equating the presentation or occurrence of a datum with knowledge. It is useless at this stage to ask how one is to go about estimating the weights of such statements as "There is an immediate thing *A*". It is more important that we conclude this critical study with a few indications of the reasons which have led Professor Reichenbach to his present "dualistic" epistemology.

If cases of perception (or immediate existence) are taken as cases of knowledge, the dualism of a subjective world (into which fall all the familiar common-sense objects) and an objective world (into which fall scientific objects) is generated in a familiar manner. For as Professor Reichenbach repeatedly illustrates, things as perceived do not exhibit properties which things taken as scientific objects are defined to have or are shown to possess by further inquiry. Thus, the perceived optical convergence of rails does not reveal either their mechanical parallelism or their optical parallelism from a different perspective; the observed smoothness of the table-top does not show the complicated molecular structure of the wood; and in general, the presentation of a trait in observation does not disclose the complex of objects which competent scientific investigation establishes as the cause or condition of the existence of that trait. But if perception is knowledge, and if that knowledge is to be veridical, it is natural to argue that traits directly observed ought to be the traits uncovered by the inferential procedures of the sciences. Since, however, this is not the case, and since the ex-

planatory power of scientific objects is obviously superior, it is but a short step to the conclusion that perceptual knowledge is "false in a certain sense", that illata have objective existence, and that matters directly observed have only subjective existence, especially since the occurrence of perceivable qualities is so clearly conditioned by physical and physiological processes in the perceiving organism. Thus, almost by a play upon words, we are condemned to inhabit a "substitute world", with only problematic means of communication with the objective world of science. Will not Professor Reichenbach be persuaded to abandon the premisses from which his dualism follows, and adhere consistently to his genuine insight that thinking as an inferential process is carried on in the objective common-sense world of houses and trees and wars, that it terminates in knowledge of this world, and that immediate existents are not objects but instrumentalities of such knowledge? Will he not reconsider his thesis that immediate existents are historical and psychological primitives, and come to think of them as *terminal points* in analysis, discriminated for the sake of making more reliable the application of our theories and of the controls we can institute to test their worth? Will he not consistently recognize, what he does so excellently on occasion, that the identification, differentiation, and location of immediate existents can be performed only in terms of things whose objectivity must be taken for granted? He may then find that the "problem" of adjusting his two worlds, like the problem of the external world, requires no solution because there is no problem to be solved.

One final point. It is not possible to know how seriously Professor Reichenbach intends his language to be taken, and one may easily do him an injustice by taking his words more literally than he planned. But however that may be, he does talk of obtaining a "completely true description" of the world in terms of organizing different subjective "pictures" or "perspectives" into one superior whole. Now it is one thing to organize into a unity the different visual perspectives of a pair of rails; it is another thing to organize into a systematic whole the various physical and chemical theories we may have for the constitution of steel; and it is a third thing, and it seems to me a gravely

confused thing, to try to organize into a unified "description" the visual perspectives *and* physico-chemical *conditions* for the occurrence of these perspectives. Rightly or wrongly, Professor Reichenbach's readers do get the impression that according to him a "true picture of the world" *would* be a *picture* or *image*, but a picture without perspectives, without focus, and without selection. Would not such a "description" simply *be* the whole world all over again, duplicated to serve a philosopher's fancy? And in any case, is he doing a service to anyone in maintaining that it is not *logically* impossible to *see* atoms or radio waves? It is a question of fact into which we need not enter whether terms like "atom" are not so defined and used that it *is* a logical absurdity to suppose that atoms could be perceived. What is relevant to our purpose is to point out that Professor Reichenbach talks of scientific objects as if *knowledge* of them were a kind of *seeing*, and that in so doing he has not clarified what terms like "atom", "radio wave", "light wave" *mean* because he has not exhibited how such terms are *used* in theoretical physics and scientific inquiry. Thus, his defense of the "independent existence" of atoms is effective enough against those who deny such existence; but both the defense and the offense are carried on upon the common assumption that the atomic theory is a pictorial report or description of matters which, alas for our human limitations, are not accessible to direct observation through the senses. Is it not high time to challenge this assumption and to insist that having knowledge is nothing like having an image, however grand and unified such images may be? And is not the way to discover what knowledge of the existence of atoms and light waves is, that of examining the *procedures* of the physicist when he claims such knowledge and of the *uses* he makes of these terms in his inquiries? Having now made his venture into epistemology, will not Professor Reichenbach return to the kind of analyses he was performing when he wrote his *Axiomatik der Raum-Zeit Lehre* and his *Philosophie der Raum-Zeit Lehre*? It is in such works that the strength and virtue of scientific philosophy lie.

Columbia University.

DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

THE HYPOTHESIS OF DUALISM

Dear Sir:

In his article,[1] *A Re-examination of Some Arguments for Realism*, Professor Taube refers to the rôle of physiology in the theory of perception. It has often been argued that since perception depends on a perceiving organism, the datum of perception can not be assigned a place in the external world. In reviewing criticisms of this argument, Professor Taube quotes H. H. Price, who declares that physiology is based on perception of external things and therefore can not be employed to demonstrate that what we perceive is not a part of, but is caused by, the external world. In order to supplement Professor Taube's stimulating analysis I shall comment on the problem of perception in the light of general principles of scientific procedure.

The dualistic theory of perception is based on the constructive hypothesis that perceptions are caused by independent things which radiate influences to the perceiving organism. Causality may be interpreted as functional relationship between thing and percept, but even with this restriction the hypothesis is not capable of direct confirmation. It is confirmed by its success in explaining past perceptions and in predicting future ones.

The physiological investigation of the dependence of perception on conditions of an organism presupposes the existence of the organism perceived and used by the experimenter. On the dualistic hypothesis the organism is assumed to be an independent reality. It is not necessary to assume that perception constitutes direct knowledge; it is a sufficient hypothesis that perceptions have objective reference. The

[1] Philosophy of Science, Oct. 1938.

experimental conclusion that perception depends on the organism is therefore consistent with the dualistic interpretation of the hypothesis of the existence of the perceiving organism. A similar procedure is exemplified in physics. In order to study the exchange of heat between two bodies the physicist uses a thermometer in measurements of temperature. But the function of a thermometer depends on the hypothesis that heat is exchanged between it and the body whose temperature is measured. The general hypothesis embodied in the thermometer is consistent with conclusions of a detailed investigation into heat exchanges. Likewise, the hypothesis that perception of external things is dependent on the organism is consistent with the detailed results of physiology.

In general, the dualistic theory may be viewed as a scientific hypothesis which explains and predicts perceptions. Contemporary physical theory is characterized by the dominant rôle played by constructive hypotheses. Assuming for the moment that everyday things are directly given in perception, knowledge of the entitles of atomic physics consists in the acceptance of hypotheses from which it is possible to deduce consequences that can be tested by experiment. The energy levels of the atom are the objects of hypotheses from which one can predict the positions of spectral lines on a photographic plate. The hypotheses concerning elementary particles predict perceptible phenomena in vacuum tubes and cloud chambers. In all these examples there is no direct perception of the entities considered; confirmation of the hypotheses consists in the explanation of past phenomena and the prediction of future phenomena. The theory of dualistic realism merely extends the method of hypothesis to the phenomenon of perception of everyday things. But no doubt is thereby raised concerning the existence of the things for which direct perception had previously been assumed. Like the hypotheses about atomic entities, the hypotheses of the independent existence of perceptible things of daily life are confirmed through the explanation of past data and the prediction of future data.

I shall not attempt to decide whether or not the dualistic hypothesis provides the best foundation for philosophy of science. However, the following quotation from Helmholtz's historic address "Die Tatsachen in der Wahrnehmung" may be of interest.[2]

"The realistic hypothesis . . . trusts the deliverance of ordinary

[2] Schriften zur Erkenntnistheorie. Edited by Hertz and Schlick, p. 129. Berlin 1921.

self-observation, according to which changes of perception subsequent to action have no psychical connection with the antecedent volition. It expresses the view, apparently confirmed by perceptions of daily life, that the material world outside us exists independently of our representation. The realistic hypothesis is undoubtedly the simplest that we can construct, is tested and confirmed in extraordinarily wide fields of application, is sharply defined in all details and is therefore extraordinarily useful and fruitful as a basis for action. The regularity in our sensations could be expressed idealistically hardly otherwise than by saying, "Conscious contents which have the character of perception proceed as if the world of corporeal things assumed by the realistic hypothesis actually exists." But we can not pass beyond this "as if"; we can not acknowledge the realistic view for more than an exceptionally useful and precise hypothesis; we are not permitted to attribute necessary truth to it, since irrefutable idealistic hypotheses are also possible."

V. F. Lenzen

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REVIEWS AND NOTES

The Synthesis of Science and Religion. By Frederick Kettner. The Biosophical Institute, 23 West 87th St., New York. 22 pages, 10¢. "Biosophy is the science of the inner life."

Research and Statistical Methodology Books and Reviews of 1933–1938. By O. K. Buros. Rutgers University Press, New Brunswick, N. J. 100 pages, $1.25.

This book will be a great help to those who wish to be guided to the newer knowledge of statistical methods. By all means let us have more of these summaries.

W. M. M.

An Introduction to the History of History. By James J. Shotwell. Columbia University Press, New York. 407 pages, $3.75.

A revised edition. Clear, fair. Tells how we are checking up on Herodotus now by his own antiquity.

W. M. M.

Denker der Italienischen Renaissance. Gestalten und Probleme. (Thinkers of the Italian Renaissance. Figures and Problems.) By Hoenigswald, Richard. Verlag Haus zum Falken. Basel 1938. 248 pages, 12 fcs, Swiss, unb.

This is the first larger publication of Hoenigswald since he has been exiled from the university-chair by the Nazi-government. It is a delightful, unacademical description of the personalities and their main philosophical ideas of the great high-period of European culture beginning with Ficinus and Mirandola and finishing with Bruno, da Vinci and Galilei. A rare book worthwhile translating into English.

ERNST HARMS

Washington, D. C.

Geist und Sein. (Mind and Being.) By Schmalenbach, Herman. Verlag Haus zum Falken, Basel, 1939, 328 pages, cloth. 18 Fcs, Swiss.

The author, professor in the University of Basel, offers a kind of new epistomology which places itself between phenomenology and descriptive psychology, somewhat reminiscent of the personalism of William Stern; but on the other hand showing dependence on Brentano and Meinong.

The book is in three parts. The first chapter, "Intentionality and Dynamics," studies the wider field of perception. The second chapter, "Symbol and Logos," tries a kind of phenomenology of the mental sphere of our consciousness as the perception of the Logos. The third chapter, "Problems of Metaphysics and Anthropology" makes the attempt to understand the interaction of both spheres of existence in man. The problem of man is to pass from a mean to a perception of the reality. A real understanding of the human consciousness shows that he is able to do this.

Ernst Harms

Washington, D. C.

Drei Vorträge über Philosophie und Parapschologie. Von Johannes J. Poortman. Leiden, 1939. A. W. Sijthoff's Uitgeversmij N. V.

In these three lectures the author hopes to convince himself and the reader that Parapsychology, known in English-speaking countries under the name of "Psychic Research," is coming of age as a science. Save for exclaiming "Eine neue Wissenschaft, eine Aufgabe!" (p. 46) he gives no clue as to the experimental procedure and postulational formulation of the "science." He is no more convincing than his godfathers, T. K. Oesterreich and G. Heymans. On the whole it belongs to the realm of literary theosophical writings.

Irwin Biser

Critique de la Mesure. By G. Beneze. Actualities Scientifiques et Industrielles. Paris, 1937. Hermann and Co., Editors. 54 pages. Price 12 Fr.

The author first discusses the antinomy of the measure of lengths and its solution by the ordinary scientific procedure which is clearly described. He then finds the origins of this measure in the kinesthetic feelings by an introspective analysis. Finally, he presents the "postulates" of arithmetic (under this name he understands rather the primi-

tive concepts of mathematicians) as results of length measurements. He indicates how other measurements can be reduced to length measurements and considers this reduction as absolutely necessary without discussing the possibility of zero methods which are often as exact and simpler.

KAROL MEISELS

Essai sur les Definitons Experimentales des Operations Chimiques. By Paul Renaud. Actualites Scientifiques et Industrielles. Paris, 1936. Hermann et Co., Editors. 38 pages. Price 10 Fr.

The author discusses a very elegant definition of chemical elements and compounds without any gravimetric consideration only by means of the chemical operations used to transform them. He then outlines the problem of study and classification of the operations by means of the elements and compounds and then considers e.g. the possibility of defining operational elements and eventually some operational atomic weights. The author does not pretend to solve the problem he poses.

KAROL MEISELS

ANNOUNCEMENT

A journal of the type of *Philosophy of Science* must rely on the ordering of reprints by authors for a significant part of its income. It is with great regret, therefore, that we withdraw the policy of furnishing twenty-five reprints gratis, and replace it with the requirement that authors must order 100 reprints at a cost of $1.00 per printed page. They may get additional reprints at a lower price which will be indicated on special order slips sent with proof. The increased cost to an author who would normally order 100 reprints is small.

The new regulation comes into effect with the next issue, July, 1939.

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